FISCAL YEAR 1986
DEPARTMENT OF ENERGY AUTHORIZATION

(Magnetic Fusion Energy and Departmental Administration and Supporting Services Activities)

HEARINGS

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND PRODUCTION

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

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CONTENTS

WITNESSES

March 8, 1985:

Page
- Dr. Alvin W. Trivelpiece, Director of Energy Research, Department of Energy, accompanied by Dr. John F. Clarke, Associate Director, Office of Fusion Energy, Department of Energy 3
- Hon. Mike McCormack, former U.S. Representative 32

March 18, 1985:

- Statement for the record from Dr. Noah Kershkowitz in behalf of the University Fusion Association 52
- Statement for the record from Peter H. Rose, president, Spectra Technology, Inc 58
- Statement for the record by the Atomic Industrial Forum 61
- Hon. Fortney H. (Pete) Stark, Member of Congress from California, on behalf of the fiscal year 1986 fusion energy budget and the Lawrence Livermore National Laboratory 64
- Dr. Ronald R. Parker, associate director, MIT Plasma Fusion Center and head, Alcator Group 71
- Dr. Tihiro Ohkawa, vice president, GA Technologies, Inc 84
- Dr. Stephen O. Dean, president, Fusion Power Associates 92
- Leonard F.D. Reichle, Magnetic Fusion Advisory Panel 99
- Joseph Gavin, chairman of the executive committee, Grumman Corp., Committee on International Cooperation in Magnetic Fusion, National Research Council 117
- Dr. Ronald C. Davidson, chairman, Magnetic Fusion Advisory Committee and Director, MIT Plasma Fusion Center 136
- Dr. Allan T. Mense, Institute of Electrical & Electronics Engineers, Inc 163
- Dr. Peter Staudhammer, manager, Energy Systems, TRW, Inc., Redondo Beach, CA 188
- Paul M. Koloc, president, Prometheus II, Ltd 199

APPENDIX I

Questions and answers from Hon. Martha Hesse Dolan, Assistant Secretary for Management and Administration, U.S. Department of Energy, regarding the 1986 budget request for Departmental Administration and Supporting Services Activities 209

APPENDIX II

SUPPLEMENTAL QUESTIONS AND ANSWERS

MARCH 8

Dr. Alvin W. Trivelpiece, Director of Energy Research, DOE 231

MARCH 18

- Dr. Tihiro Ohkawa, GA Technologies 278
- Dr. Stephen O. Dean, Fusion Power Associates 282
- Leonard Reichle, Magnetic Fusion Advisory Panel 285
- Joseph Gavin, Committee on International Cooperation in Magnetic Fusion 288
- Dr. Allan T. Mense, Institute of Electrical and Electronics Engineers 292
- Paul M. Koloc, Prometheus II, Ltd 296
- Dr. Ronald R. Parker, MIT 309
- Peter Staudhammer, TRW, Inc 314

1986 DEPARTMENT OF ENERGY AUTHORIZATION

FRIDAY, MARCH 8, 1985

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Research and Production,
Washington, DC.

The subcommittee met, pursuant to call, at 8:30 a.m., in room 2318, Rayburn House Office Building, Hon. Marilyn Lloyd (chairwoman of the subcommittee) presiding.

Mrs. Lloyd. The Subcommittee on Energy Research and Production will come to order.

First of all, I ask unanimous consent that the subcommittee permit today's proceedings to be recorded and covered by the media and other persons. Without objection, it is so ordered.

Good morning, ladies and gentlemen. The Energy Research and Production Subcommittee will receive testimony today from the Department of Energy on the fiscal year 1986 budget request for the Magnetic Fusion Energy Program. From my perspective, there are three major issues to be considered in today's hearing. The first is: Having suffered reductions of 20 percent over the last 2 years, can the DOE pursue a strong Magnetic Fusion Energy Program without making some drastic changes to reorient the program? We shall pursue the related question of whether some additional funding might provide the Department with critical programmatic flexibility.

Although all of these major issues are affected by the budget levels, the remaining two issues are inextricably linked to one another. These issues are: The prospects for improving the MFE Program focus through an ignition experiment and the overall question of international cooperation.

It appears most important that the MFE Program have a focus for development and technology beyond TFTR and other operating experimental devices. The community is virtually unanimous that this should be an ignition device, in other words, one which produces a burning plasma. We must understand whether it makes programmatic sense for that to be a lower cost device, under $500 million, or should the DOE go for a much more ambitious project? I, for one, believe that the domestic program could afford a more modest ignition device within projected budgets, whereas larger projects—closer to test reactors—can clearly only go forward with significant international cost-sharing.

Thus, the ignition choices drive the international cooperation approach since it will take 3 to 5 years to arrive at a multinational agreement to build such a device. Thus, it seems that while the United States should welcome international cost-sharing for a low-cost burner project, it should not be a requirement for a go ahead in the near future. I am very interested in what the Department may have to say about these matters this morning.

Dr. Trivelpiece, I am glad to welcome you back as well as our old friend, Dr. John Clarke. Good to see both of you. Your written testimony will be included in the hearing record. You may summarize or proceed as you wish. I would now like to recognize the ranking minority member, Mr. Morrison.

Mr. MORRISON. Thank you, Madam Chairman.

Good morning. We are off into another subject area this morning. I look forward to this very much. I feel strongly about the potential of fusion programs. If it is permissible, Madam Chairman, I would like to have my complete statement part of the record because I extol the virtues of fusion, which take up time, and I know you have an airplane to catch. I want to stay on your good side.

[The prepared opening statement of Mr. Morrison follows:]

OPENING STATEMENT OF HON. SID MORRISON ON THE FISCAL YEAR 1986 DOE AUTHORIZATION REQUEST FOR MAGNETIC FUSION ENERGY PROGRAMS

Good morning. Today we begin our hearings on the DOE Authorization Request for Magnetic Fusion Programs. The fusion R&D programs are exciting because they are aimed at fulfilling a dream: harnessing an essentially unlimited source of energy. Many have wondered through the ages about harnessing the energy of the Sun—trying to bottle the Sun if you will. If we could develop the technology which would allow us to safely confine the incredibly hot plasma, we would have an inexpensive, abundant, and indigenous energy source. This is what our fusion R&D programs are all about.

While we can all dream about the limitless possibilities fusion energy promises, today we seem to be light-years away from a practical fusion energy technology. We are in much the same position we were 30–40 years ago during the dawn of controlled nuclear fission development. Since we are on the cutting edge of fusion development, it behooves us to discern the proper focus of a long-term R&D program. It is critical that we fashion a well-defined and focused mission for fusion energy development; the vacilation that has characterized the program the past few years must cease. I look forward to learning more about the proposed de-emphasis on development specific fusion facilities in an exchange for an emphasis on scientific and technological fundamentals essential to future fusion applications.

The future mission plan must also be consistent with responsible budget planning. The fiscal year 1986 request for fusion is already 11 percent below last year's level. The surprise budget cuts last year successfully got the attention of Mr. Trivelpiece and the Department. I hope that this year's budget can be developed in close harmony with DOE and can reflect a consistent and clearly articulated mission.

Madam Chairman, today's taxpayers and our future generations which will rely on this bottled energy from the Sun, deserve nothing less.

Mr. MORRISON. I would like to include some feelings that I have as an outsider looking in, and perhaps my misconceptions can be corrected.

While we can all dream about the limitless possibilities fusion energy promises, today we seem to be light-years away from a practical fusion energy technology. We are in much the same position we were 30 to 40 years ago, during the dawn of controlled nuclear fission development. Since we are on the cutting edge of fusion development, it behooves us to discern the proper focus of a long-term R&D program. It is critical that we fashion a well-defined and focused mission for fusion energy development; the vacillation that has characterized the program the past few years must cease. So, I look forward to learning more about the proposed deemphasis on developing specific fusion facilities in an exchange for an emphasis on scientific and technological fundamentals essential to future fusion applications.

The future mission plan must also be consistent with responsible budget planning. The fiscal year 1986 request for fusion is already 11 percent below last year's level. The surprise budget cuts last year successfully got the attention of our guests today and other folks at the department. I hope that this year's budget can be developed in close harmony with DOE and reflect a consistent and clearly articulated mission.

Thank you, Madam Chairman, for this opportunity. I look forward to the visit with our guests.

Mrs. LLOYD. Thank you very much, Mr. Morrison.

There has been one change in today's agenda. Our old friend, Mike McCormack, will testify after the DOE witnesses have been questioned.

Please proceed with your testimony.

STATEMENT OF DR. ALVIN W. TRIVELPIECE, DIRECTOR OF ENERGY RESEARCH, DEPARTMENT OF ENERGY, ACCOMPANIED BY DR. JOHN F. CLARKE, ASSOCIATE DIRECTOR, OFFICE OF FUSION ENERGY, DEPARTMENT OF ENERGY

Dr. TRIVELPIECE. Madam Chairman and Mr. Morrison, it is a pleasure to be here once again.

Since the last time I appeared before this committee and had the pleasure of doing so, I have had the displeasure of having to adjust to something in excess of a $40 million cut. Before discussing how we adjusted to it and what we did, let me set the stage for that by reminding you that in 1973 we encountered fuel shortages, oil shortages, gasoline lines, and so on. At that stage, in the then Atomic Energy Commission, the predecessor of the Department of Energy, established several plans that were looking toward the future. One of these plans included trying to lay out a program that would result in a fusion reactor being demonstrated by something like the year 2000.

The program plan that was laid out then was somewhat ambitious. It anticipated certain results. It had as one of its principal goals the idea that deuterium-tritium burning would be one of the features, and there was a development and technology program that would try to make sure that the necessary information would be available by the year 2000 so that all these things would come together.

Now, the last year then, as a result of the appropriations process, we had a $40 million cut. So, we have had to ask how would we go about accommodating that and still try to maintain a balanced program that makes the proper goals appear in something like the proper order. In order to do so, we have delayed or deferred the deuterium-tritium burning in the Tokamak fusion test reactor at Princeton, and we have slowed down and descoped somewhat the MFTFB project at Livermore and delayed some of the development and technology activities that would be required for reactor-specific things in the year 2000.

We are now confronted with a second $40 million reduction. I am not entirely sure of what the purpose of the first one was, but I do know what the purpose of the second one is. This is part of the President's desire to try to control the deficit. He imposed a cost ceiling on the Department. As part of the Department's cost reduction efforts, the Fusion Program was asked to make a reduction of about $40 million. It is more difficult, as Mr. Morrison indicated, to try to make the additional cuts on top of the surprise cut of last year.

We are still attempting to maintain some degree of balance and address the appropriate critical issues and still make realistic progress. One of the things I personally find rather ironic is that in the 1973 time frame, one of the questions was, will fusion work. And there was some debate over that. The level of achievement at that stage was such that maybe that might have been in doubt. There was a struggle to demonstrate that.

I think now we find ourselves in a slightly different situation. That is, it is pretty well agreed that, yes, it will work, but the urgency to do so has been reduced by the reduction in oil prices, the supply of oil, and, as you may have noticed, the price of yellowcake uranium ore dropped to something like $15 a pound in the middle of January from a high of only a few years ago of around $80 a pound. So, that has some influence on these things.

The Fusion Program, from my perspective, having watched it and participated in it for a number of years, has met all the goals that it has set out to do. It has done an excellent job in the research and solved a large number of very difficult problems. It has made absolutely outstanding contributions to education of a generation of scientists and engineers with theoretical and experimental skills that have been quite valuable to the Nation.

The reductions that have been made in the program this year are consistent with the other reductions that were made elsewhere in the department, that is, that fusion was not as one element of the Department of Energy singled out for particularly harsh treatment, that Secretary Hodel had to adjust to these reductions. We were, I believe, treated fairly in the context of the other activities within the Department.

That concludes the remarks I would like to make to begin with. I would be delighted to try to answer any questions you might have, Madam Chairman.

[The prepared statement of Dr. Trivelpiece follows:]

Statement of Alvin W. Trivelpiece

Director of Energy Research

Department of Energy

before the

Subcommittee on Energy Research and Production

of the

House Science and Technology Committee

March 8, 1985

Madam Chairman and Members of the Subcommittee:

I am pleased to appear before you today to discuss the FY 1986 budget request for the Magnetic Fusion Energy (MFE) program. The goal of the program is to develop the scientific and technological base required for the eventual production of fusion energy. The development of fusion power is directly in line with the goal of the National Energy Policy Plan. That goal is to foster an adequate supply of energy at a reasonable cost. Although it may take decades to develop significant new energy sources that will contribute to a more flexible energy supply system, it is important, from an economic, social and defense perspective, to continue to work steadily toward that goal.

The Fusion program's strategy is to maintain a broad R&D program with emphasis on establishing the basic elements, including components and subsystems, of the science and engineering technology required for fusion. This strategy will allow us to explore the feasibility and potential usefulness of a range of fusion device options. In doing so, the program is designed to ensure that sufficient information is available to allow private industry to make informed decisions on whether to pursue fusion development in the future. The schedule for completion of magnetic fusion energy development is directly related to the technical, political and economic uncertainties of the world energy supply that are likely to exist for several decades.

In addition to R&D supported in the U.S., there are important international dimensions to fusion research. Because of the global benefit of fusion applications, the commonality of goals, the scale and difficulty of the research and the lack of competitive commercial pressures, international collaboration in fusion energy development is particularly appropriate. The

United States Fusion program has had a successful history of international cooperation and collaboration; and we will continue to seek out mutually advantageous opportunities for collaboration that will minimize duplication and allow us to achieve our goal more effectively.

The U.S. Fusion program is pursuing three objectives, those of science, technology, and technology transfer, that must be achieved to meet the program goal. The science objective is to be able to predict the behavior of high temperature plasmas confined in different magnetic fusion configurations. There are several magnetic confinement systems that could be developed into energy-producing devices. An ability to predict how the plasma will behave is particularly important because all of these systems cannot be fully developed in parallel. Only through our ability to predict plasma behavior will we be able to make informed choices about the best direction for fusion development.

The technology objective is to develop unique fusion components that can operate under conditions relevant to fusion energy. Wherever possible, the program will draw on technological developments that are already available elsewhere and will support research and development on those components that are unique to fusion or beyond the industrial state-of-the-art. Such unique components exist in the areas of magnetics, plasma heating, energy removal, fuel production, and materials.

The third objective, technology transfer, is to provide knowledge and experience in order to identify a range of options for private sector investment and commercial development of fusion. This objective is initially pursued by having the industrial participants apply their expertise to the development of fusion components and subsystems. Through this direct participation, industry will continue to gain experience with the unique aspects of fusion. Technology transfer provides a stimulus to U.S. technology growth as well as providing a training environment for industrial scientists and engineers.

With this strategy in place, the program can move ahead to address the four key technical issues that must be resolved if fusion power is to be developed successfully. The first of these involves magnetic confinement systems. It is important to explore an appropriate range of possibilities for magnetic confinement to ensure selection of the most desirable fusion reactor system in the next century. The second technical issue is to understand the properties of burning plasmas. A fusion plasma is expected to burn when the heat released within the plasma by the fusion reaction is sufficient to maintain the plasma temperature in the face of heat loss from the system. The conditions required for ignition will be explored in the Tokamak Fusion Test Reactor (TFTR) at Princeton during the latter part of the 1980's. Following this achievement, at least one burning plasma must be produced and studied to complete the scientific base. The third technical issue is materials for fusion systems. The ultimate economics and environmental acceptability of a fusion system will depend on the development of various materials that are required. The last issue is nuclear technology for fusion systems. This includes developing a fusion "blanket" that must perform several functions including converting the energy released by a fusion reaction into useful energy, creating part of the fuel for the reactor and allowing recovery of this fuel, as well as shielding critical components from radiation. To resolve this issue, it is necessary to develop technical options in order to be able to optimize the economic, safety, and environmental performance of a fusion reactor. The energy recovery system, or blanket, will have to be tested in a fusion environment.

In addressing these issues, sufficient breadth must be maintained to ensure that adequate information is available to allow informed decisions on the desirability and schedule of the ultimate commercial applications of fusion energy. Since it is impossible at this time to predict either the economic conditions affecting such a decision or its schedule, it is premature to focus on developing a specific reactor type. Instead, the program will systematically study and resolve the basic issues of science and technology that affect the feasibility of a range of options.

To expand the science and technology base it is necessary to develop a thorough understanding of the scientific principles underlying the confinement of plasmas in magnetic fields. There are two principal approaches to magnetically confining a plasma: the toroidal and linear concepts. A number of variations are possible within each of these two categories. Since the basic scientific phenomena which must be investigated experimentally change according to the shape of the magnetic configuration, a variety of concepts will be investigated to complete the science base. By studying these related concepts, a better understanding of plasma behavior will be achieved than if only one concept were studied. Further, it is expected that this broad approach will most readily lead to improvements in present devices and the development of superior magnetic geometries for practical application in the future. This research includes work on toroidal configurations including tokamaks, reversed field pinches, stellarators, and compact toroids, as well as linear configurations called tandem mirrors.

The Magnetic Fusion program made substantial scientific and technical progress in 1984. Among the confinement concepts, the tokamak has achieved the best plasma conditions in terms of confinement time, temperature and density. Several of the program's tokamak facilities, including the Princeton Large Torus (PLT), Doublet III (D-III) at GA Technologies (GA) in San Diego, Impurity Study Experiment-B (ISX-B) at Oak Ridge National Laboratory (ORNL), the Princeton Beta Experiment (PBX), and Alcator C at the Massachusetts Institute of Technology (MIT), have been able to explore different and complementary aspects of plasma behavior at near reactor-level plasma conditions.

In addition, the Tokamak Fusion Test Reactor (TFTR) has begun its experimental research phase to investigate the physics that must be mastered in order to ignite a plasma. Experiments supported for FY 1986 in TFTR should allow study of fusion plasma behavior at energy breakeven temperature and density conditions. The energy breakeven point, at which the energy produced by a fusion plasma is equal to that required to sustain the plasma, is an important milestone for magnetic fusion.

A criterion that must be satisfied to reach this breakeven point is that the product of plasma density, energy confinement time, and temperature must be greater than a certain minimum value. This past year, scientists at GA and at the Princeton Plasma Physics Laboratory (PPPL) made significant progress toward achieving these threshold plasma conditions. At GA, a team of Japanese scientists working on the Doublet III tokamak, under a joint funding agreement between the Department of Energy (DOE) and the Japanese Atomic Energy Research Institute (JAERI), realized a world record density-temperature-confinement product which was about 30 percent of that needed for energy breakeven. In addition, initial operation of TFTR without any auxiliary heating achieved an encouraging energy confinement time of 0.38 seconds at high plasma currents. These results increase our confidence that TFTR will reach fusion energy breakeven conditions when it is operated with full auxiliary heating and a deuterium-tritium gas mixture in 1988.

Progress also has been made in mirror research. The Tandem Mirror Experiment at Lawrence Livermore National Laboratory has continued to establish the basic scientific principles of confining plasmas with combined electric and magnetic fields. In addition, a second tandem mirror research facility called TARA, has begun operation at MIT. A major 1984 achievement in research on open-ended, mirror devices was the direct experimental verification of a theoretical concept called the "thermal barrier". Thermal barriers play a key role in stopping plasma from leaking out the ends of cylindrical-shaped tandem mirror machines. Proof of the barriers' existence increases the likelihood that reactor-grade plasmas can be confined in large mirror devices.

In the technology area, progress has been equally encouraging. New, more effective beam technology, used to heat fusion plasmas, has been tested successfully. These new beam sources will improve the capability of our experiments to investigate high-temperature plasmas because the sources can operate for longer times at higher powers. In 1984, scientists using high intensity neutral particle beams to heat fusion plasmas successfully tested the most powerful neutral particle beam plasma heating system to date, achieving pulse lengths up to 30 seconds long. These beams should be sufficient for obtaining the energy breakeven plasmas expected in TFTR over the next few years. A second new heating source, high-frequency, high-power electromagnetic waves generated by a newly developed gyrotron tube, have been shown to preferentially heat the electrons in a fusion plasma, thus providing a promising additional mechanism for plasma heating and control. Extrapolation of results from successful 1984 plasma heating tests indicate that it should be physically possible to build a reactor size plasma heating system with gyrotron technology.

The plasmas in fusion machines need to be refueled as fuel is burned up or particles are lost from the plasma. One of the most promising approaches to refueling is use of pellet fuel injectors. These injectors have provided the key to obtaining stable, well-confined plasmas in current experiments such as Alcator C and Doublet III. In 1984, a pneumatic type injector, which uses a compressed gas mechanism to fire frozen hydrogen pellets at 2 kilometers per second, enabled world record plasma conditions to be reached in several fusion experiments. In addition, a centrifugal type injector accelerated single pellets to almost 1 kilometer per second, and indications are that this could be done on the repetitive basis needed for a fusion reactor. These results mean we have now developed a capability to fire fuel pellets deep into reactor-grade plasmas.

An important element of the Magnetic Fusion program is to provide a technology base for large-scale superconducting magnet systems, which exhibit virtually zero electrical resistance at cryogenic temperatures. World-wide recognition of this technology need has resulted in a major international cooperative program through the International Energy Agency (IEA), involving the U.S., Japan, Switzerland, and Euratom. The first two large superconducting coils developed under this international collaboration have been energized in the International Fusion Superconducting Magnetic Test Facility at the ORNL. In late 1984, a test with the two foreign magnets and one U.S. magnet provided an integrated checkout of subsystems as well as an initial test of interactive magnetic and structural stress effects on large superconducting magnets. These results will be used to guide a full six-magnet test schedule to begin in late 1985. Finally, new approaches to develop materials that will not become as radioactive as conventional materials for use in fusion systems have been developed.

In the area of international cooperation during the past year, cooperation with the European Community, Japan and the Soviet Union continued to play an important role in the U.S. Magnetic Fusion program. Areas of cooperation include sharing research data, joint planning of selected research programs, and joint support for major scientific and technological projects.

With the endorsement of the heads of state meeting at the summit of industrialized nations, three panels initiated in 1984 began to: 1) develop potential opportunities for collaboration through joint support of large scale projects; 2) identify increased near-term collaborative activities in physics, technology and concept development; and 3) evaluate opportunities for reducing administrative barriers to collaboration.

In 1984, the Department of Energy signed agreements with the Japanese Atomic Energy Research Institute to test equipment in the U.S. for improving tritium processing systems, and to evaluate in Japan the effects of high energy neutrons on materials. The U.S. successfully tested a key piece of equipment for the European Community's large fusion facility, the Joint European Torus (JET), and also began to work with Spain on advanced stellarator development. Technical agreement was reached on three new agreements with the International Energy Agency in the areas of large tokamak facility operations, stellarator experiments and impurity control in fusion plasmas.

FY 1986 Budget Request

The FY 1986 budget request for Magnetic Fusion Energy is $390.0 million. The request includes $346.4 million for Operating Expenses, $30.4 million for Capital Equipment, and $13.2 million for Construction (Table 1).

This budget request will provide support for scientific experiments necessary to understand the behavior of fusion plasmas confined in magnetic fields. In addition, the supporting technology development needed to carry out the scientific program and to advance the state-of-the-art in those

Table 1
Magnetic Fusion Energy
Budget Authority
($ in millions)

	FY 1985 Estimate	FY 1986 Request
Operating Expenses	$374.3	$346.4
Capital Equipment	30.2	30.4
Construction	32.5	13.2
Total	$437.0	$390.0

areas of science and technology required to realize the unique features of fusion as an energy source will be funded. Support also will be provided to continue to execute international agreements.

The budget request for FY 1986 reflects a significant shift in the program balance. The near-term scientific and technological output of the program will be emphasized while continuing to pursue the long-range goal of developing fusion at a pace consistent with our national energy needs. The near-term program achievements also will continue to include the production of well-trained scientists and engineers and contribution of advanced technology developments for industrial and defense applications.

The Magnetic Fusion Energy budget is organized into five interrelated functional areas that form the basis of a unified and balanced effort to conduct the research and development necessary to define a practical fusion product. The Confinement Systems and the Applied Plasma Physics subprograms, working in conjunction, provide the physics base for the Magnetic Fusion Energy program. In the near-term, the achievement of plasmas with the characteristics required to produce fusion energy flow from the experiments conducted in the Confinement Systems subprogram, with its dual focus on toroidal confinement systems and mirror confinement systems. The Applied Plasma Physics (APP) subprogram develops plasma physics theories and models to predict and explain the experimental results and suggest directions for additional investigations. Based on this information, the APP subprogram also initiates experimental and theoretical investigations of basic physics problems related to confinement concepts and develops diagnostic instruments for use by the Confinement Systems subprogram.

Using information developed by these two subprograms, the Development and Technology subprogram develops components and systems for use in near- and mid-term fusion experiments, and long-term generic materials. This subprogram also performs conceptual design studies for fusion power reactor concepts and next generation devices. These design studies identify component and system development needs that must be undertaken to prepare for the eventual application of fusion.

In carrying out the assignments of the physics and technology tasks, needs for major new facilities are identified from time to time. The Planning and Projects subprogram provides for construction of major new facilities and fabrication of auxiliary components for these facilities. Finally, the Program Direction subprogram includes the funding to cover salaries, benefits, travel, and associated expenses of the Headquarters Office of Fusion Energy. The proposed budget levels for these areas are indicated in Table 2.

This budget request reflects the continuation of the program's reorientation that was initiated in FY 1985. TFTR will proceed at a more deliberate pace, and operation of Mirror Fusion Test Facility-B (MFTF-B) at Lawrence Livermore National Laboratory (LLNL) will be delayed until an adequate scientific base is available to make effective use of the facility. Initial tritium experiments on TFTR have been delayed until FY 1989 so that high-temperature, fusion-quality plasmas can be studied more thoroughly. By the end of 1986, we expect to achieve the equivalent of energy breakeven conditions in a deuterium plasma in TFTR. This should provide the information that would let us predict the requirements for achieving

Table 2
Magnetic Fusion Energy
Budget Authority
($ in millions)

	FY 1985 Estimate	FY 1986 Request
Operating Expenses		
Confinement Systems		
Toroidal Confinement Systems	$151.1	$147.4
Mirror Confinement Systems	35.9	37.5
Applied Plasma Physics	82.5	78.1
Development and Technology	67.9	62.5
Planning and Projects	32.8	16.9
Program Direction	4.1	4.0
Subtotal	$374.3	$346.4
Capital Equipment	30.2	30.4
Construction		
Fusion Materials Irradiation Test Facility	3.0	--
Mirror Fusion Test Facility-B	20.1	4.1
General Plant Projects	9.4	9.1
Subtotal	32.5	13.2
Total	$437.0	$390.0

ignition with a deuterium-tritium plasma. The startup of the MFTF-B facility will be postponed to allow a more thorough exploration of tandem mirror scientific principles in smaller mirror devices. The FY 1986 funding will support the postponement over the next two fiscal years of contracts for auxiliary components.

Several experimental tokamak devices will continue operation in FY 1986. Doublet III at GA Technologies in San Diego will begin operation under a U.S.-Japanese cooperative program. It will explore the limits of high beta operation through the use of elongated D-shaped plasmas. Alcator C at MIT will operate to provide physics data on the heating of high density plasmas, a basic factor in establishing the feasibility of an affordable ignition and burn experiment.

Investigation of toroidal improvements will be pursued with the completion of fabrication activities on the Advanced Toroidal Facility at ORNL and full operation of the Poloidal Beta Experiment at Princeton.

In the mirror program, researchers at the Lawrence Livermore National Laboratory will conduct experiments at the Tandem Mirror Experiment (TMX) facility to address the key physics issues of standard thermal barrier tandem mirrors. In addition, the TARA device at MIT and TMX will be used to investigate innovative mirror configurations that have the potential to improve linear concepts.

In the technology area certain long-term technology development activities will be deferred or reduced to allow concentration on near-term technologies needed to advance scientific understanding. The International Fusion Superconducting Magnet Test Facility at ORNL will fulfill our commitment to test six large superconducting magnets in a toroidal array. This will establish the technology needed to provide efficient, steady-state magnetic fields to confine plasmas for the tokamak concept. Development efforts will continue on pellet fuel injectors to support present fusion devices, and on ion cyclotron resonant frequency (ICRF) and electron cyclotron resonant frequency (ECRF) heating components to provide plasma heating systems for present and future fusion devices. Materials development needed to meet the unique requirements of fusion systems will continue with emphasis on developing coatings for high heat flux, materials for plasma interaction applications, and materials that will withstand radiation damage effects while concurrently producing little residual radioactive material. Exploration of copper magnet machine designs for ignition experiments and research on fundamental new materials will continue. However, to support these priorities, efforts on superconducting high field magnets will be stretched out, the neutral beam test facility at Lawrence Berkeley Laboratory will be shut down, and efforts in the area of tritium technology processing will be reduced. These efforts will be resumed when necessary to complete the fusion science and technology base.

Increased emphasis will be placed on studying some of the more fundamental aspects of fusion science. Research will be undertaken to exploit creative new ideas in both the reversed field pinch and compact toroid confinement concepts. These experiments should provide insights to enhance our understanding of how to better create and control a fusion plasma. Support of theory and small scale experimental plasma research will be decreased resulting in a reduction in the level of university support in these areas. The leasing of a new CRAY 2 computer for the Magnetic Fusion Energy Computer Center of Lawrence Livermore National Laboratory will continue to increase the computational capacity and capability of the Center which is used by fusion researchers throughout the program.

The request for Capital Equipment is $30.4 million and will provide the necessary hardware to support the planned experimental program. The Construction request of $13.2 million provides support for the program's General Plant Projects needs ($9.1 million) and completion of the construction activities on the MFTF-B project ($4.1 million).

Madam Chairman, that concludes my testimony. I would be happy to answer your questions.

U.S. Department of Energy
Assistant Secretary for
Congressional, Intergovernmental
and Public Affairs
Office of Public Affairs
Washington, D.C. 20585
202/252-5644

BIOGRAPHY

ALVIN W. TRIVELPIECE

Dr. Alvin W. Trivelpiece was nominated by the President on July 8, 1981, and confirmed by the Senate on July 27, 1981, as Director of the Office of Energy Research, U.S. Department of Energy (DOE). In this position, Dr. Trivelpiece serves as technical adviser to the Secretary on the Department's energy research and development programs and is responsible for the multipurpose laboratories and energy education and training activities. In addition, he manages DOE's programs for basic energy research, health and environmental research, high energy and nuclear physics, and magnetic fusion.

Dr. Trivelpiece has extensive experience in the areas of plasma physics and fusion research. From 1978 until assuming his current position, Dr. Trivelpiece was Corporate Vice President of Science Applications, Inc., of La Jolla, California, primarily responsible for internal research programs relative to innovative technical developments. From 1976 to 1978, he was Vice President for Engineering and Research at Maxwell Laboratories, San Diego California.

During the period 1973 to 1975, Dr. Trivelpiece served with the U.S. Atomic Energy Commission as Assistant Director for Research in the Division of Controlled Thermonuclear Research. Prior to joining the Federal Government, he was Professor of Physics at the University of Maryland, serving in that capacity from 1966 to 1976. From 1959 to 1966, he was a professor at the University of California at Berkeley in the Department of Electrical Engineering.

He received a B.S. degree from California Polytechnic State University in 1953, an M.S. degree in 1955, and a Ph.D. degree in 1958. Both advanced degrees were awarded by the California Institute of Technology.

The recipient of several honors and awards, Dr. Trivelpiece is listed in *American Men and Women of Science*, *Who's Who in America*, and *Who's Who in the World*. He holds several patents in the area of physics research and is the author and coauthor of over 100 technical reports and books.
Dr. Trivelpiece is a member of numerous professional organizations and is a fellow of the American Physical Society, the Institute of Electronics and

Electrical Engineers, and the American Association for the Advancement of Sciences. He has served as a consultant to various private associations and Government agencies and has lectured at conferences and universities throughout the United States and abroad.

Dr. Trivelpiece was born in Stockton, California. He is married and has three children.

June 1983

Mrs. LLOYD. Thank you very much.

You know, when we look at the budget that has been reduced again by $40 million, it seems like we really can't do what we need to do with these annual reductions, that we really are diminishing our focus.

The budget mentions revised priorities for fiscal year 1986, such as the upgraded version, field pinch, the compact water experiments, and the Tokamak improvements areas, and increase in the use of supercomputers to solve the fusion problems. While the committee has no objection to these changes in priorities, what lower priority work is being deferred?

Dr. TRIVELPIECE. I am not sure that lower priority is exactly the right term. We have descoped and delayed some of the activities with the MFTF-B project. But in the sense of lower priority, it is those development and technology activities that would produce information required to do the more engineering aspects of reactor development needed for the year 2000. So, to the extent that those could be delayed and deferred, they have. It has been difficult to try to adjust within the other elements of the program to accommodate these cuts.

Mrs. LLOYD. Has there been a stretchout?

Dr. TRIVELPIECE. There has been a stretch-out in some activities. The MFTFB has been stretched out, and some of the development and technology information generating activities have been deferred.

Mrs. LLOYD. What could be done to consolidate or perhaps to streamline the fusion, the search and development, so that we can reduce costs without losing the momentum that we hope to achieve and the opportunity to shorten the time for a test reactor?

Dr. TRIVELPIECE. That is an excellent question, for which I don't have a good answer, how to streamline the program. We have done this, so to speak, in the breach of trying to find a way to maintain the core elements of the program, the excellent parts of it that are needed to make progress, to slow down certain construction activities so that the scientists and engineers who are involved in the activities can maintain some momentum. It is not an easy process. Streamlining——

Mrs. LLOYD. In other words, we really can't streamline it?

Dr. TRIVELPIECE. I find it very difficult to find a way to propose how we could streamline it so that the same goals and objectives could be met in the same time schedule with these reduced funds.

Mrs. LLOYD. Is there a point of diminishing returns on research and development, in other words, alternative and advanced magnetic fusion confinement systems, without the guidance that a low burn experiment could provide?

Dr. TRIVELPIECE. If by that you mean, what would we do with additional reductions, obviously we would have to reorganize the program in an entirely different manner.

I am not sure I understood the thrust of your question precisely.

Mrs. LLOYD. Are we not getting return on our Research and Development Program without a low level burn experiment?

Dr. TRIVELPIECE. I think we are getting excellent return in the sense that the science and technology needed to provide the database to make progress is being generated.

Mrs. LLOYD. What work is being planned for the TFTR in fiscal year 1986 that will lead to plasmas closer to reactor relevant conditions?

Dr. TRIVELPIECE. In adjusting to the $40 million reduction last year—and I say, since we have testified before you last time, the activities there have been reorganized in such a way so that the deuterium-tritium burning, which had been scheduled early, has now been deferred in favor of doing certain experiments in hydrogen that will permit demonstrating the plasma conditions early on. Now, this was essentially a reordering process and not a descoping of the program. The costs of introducing the apparatus, the remote handling equipment, and so on, necessary to introduce tritium and do experiments that would generate a reasonable supply of neutrons is rather expensive. So, rather than doing some of these things early to demonstrate the production of neutrons, those activities have been delayed in favor of doing some of the hydrogen experiments early on to demonstrate the plasma conditions and once and for all establish that the fusion reactor plasma conditions can be generated in the experimental apparatus.

Mrs. LLOYD. Well, of course, scientific excellence is certainly necessary to maintain a strong Fusion Energy Program. Without it, I don't think we can make any major achievements toward the peaceful use of fusion energy. I am sure you will agree with that.

Will there be any synergism between the TFTR experiments and the Big D work, when Big D goes back on line? If so, would you describe that for us?

Dr. TRIVELPIECE. I think I would like to turn to Dr. Clarke for an answer to that question. I think the answer is yes, but perhaps he would care to elaborate on that yes.

Mrs. LLOYD. Dr. Clarke?

Dr. CLARKE. As Dr. Trivelpiece has indicated, the purpose of the work on the TFTR is to establish the plasma conditions for ignition. That is a prerequisite for any next step in that direction.

The Big D experiment, on the other hand, looks a little bit further out and is attempting to produce plasma conditions that would be more applicable to practical development in the long run.

Mrs. LLOYD. Thank you.

Mr. Morrison?

Mr. MORRISON. I am interested in pursuing one item in question, the potential for international cooperation. I know it has been something that we have looked forward to. I am just wondering if you could comment on even such programs as a parochial one for me, the fusion materials irradiation test facility. I am wondering even within your restricted budget if there is any possibility for FMIT if we had international cooperation out there at a reachable distance.

Dr. TRIVELPIECE. The activities in international collaboration and cooperation in fusion, I think, have been excellent. It is one of the scientific and technical programs that has been characterized by a strong interest on the part of scientists from many countries to cooperate and collaborate in various kinds of experimental activities.

Even so, there has been an element of competition that has characterized that interaction. It has led to, in the present circumstance, three major Tokamak experiments: One in England called

JET and one in Japan that will be dedicated later this year called JT60, and the TFTR at Princeton. Those Tokamaks, as they now exist, will produce results, which while not identical, are certainly in the same general parameter range. I think a great deal of useful synergism does result from scientists from the various countries being involved in these projects in a cooperative manner.

However, the issue of international collaboration has been focused more intensely recently as a result of Mr. Mitterrand's suggestion in the Versailles summit process that there would be some 18 areas for science and technology cooperation, recognizing science and technology as one of the engines of the world's economic growth. The United States shares the co-lead with European communities in terms of the working group that was established through that process to view what it is that needs to be done in international collaboration in fusion.

Following the London summit last year, the group of summit nations that are involved in this met in Brussels. I chaired the activity along with my cochairman, Dr. Fasella, from the European commission. In that meeting we organized essentially three groups of activities. These three groups of activities could be characterized as the wholesale, retail, and administrative issues activities. By wholesale, what we meant and what we have asked the scientists and others involved in this to do is to examine what are the next steps. If you could control perfectly the entire course of the Fusion Program and could say that we need this next and this next and this next, could you do it in such a way that country A does thing one and country B does thing two.

But the idea is clear: can you have a cooperative program around the world so that there is not the duplication of essential large facilities? So that group is meeting. They have delivered a report. We will make that report available to the Bonn summit in the near future.

The next was the retail. Within the present envelope of things, can we do a better job of cooperation? I think the answer is yes, and the individuals involved are seeking to do it.

Another interesting aspect was the administrative issues, that is, if the heads of state are truly interested in seeing international cooperation take place, then perhaps they could assist in trying to reduce some of the impediments that administrative things cause, such as customs regulations, visas, spousal work permits, and so on. So, I think that is going well.

To come more specifically to an answer to your question, in that climate what we have tried to do is to identify several projects that would be worth the international collaboration, the things where the science and technology need not be duplicated in any way, that the information could easily be made available to all concerned. The Fusion Materials and Irradiation Test facility was clearly one of those objects.

We worked rather diligently in trying to cooperate with the European communities and with the Japanese to come to an agreement where they would contribute some of the funds and we would contribute some of the funds, and we would operate the facility. That project, I am afraid, was one of the early casualties of the $40 million cut. Up until that time, we had been able to maintain the position that the United States would be in a position to make its share of the funding for that facility available, subject to the availability of funds from the Europeans and Japanese.

I think we were very far along in the negotiations on that, but our position was somewhat compromised by the $40 million cut. At that stage it did not seem possible to me, within the other envelope of things needed to be done, that we could carry on that project. But I would be interested in learning if you have some suggestions as to how we might be able to do it. I think it was the precursor of the kind of cooperation that to me characterizes what needs to be done on a worldwide basis to make effective progress in fusion and not have duplicative efforts take place.

I apologize for such a long answer.

Mr. **Morrison**. I appreciate that, because I know that there have been significant efforts in international cooperation. I just have the sense that the United States has to be somewhat suspect as we grind though these meetings, if we are constantly pulling the financial rug out from under you in those efforts.

I would also like to ask if your current deemphasis on demonstration projects—and you have mentioned them—is consistent with the intent of the 1980 Magnetic Fusion Energy Act, which was both fathered and mothered by our good friend, Congressman McCormack.

Dr. **Trivelpiece**. The act and its intent are laudable. It was created at a time in which we were still concerned with a rapid growth of the price of oil and a diminishing supply, and so on. Subsequently, however, the conditions have not proved to be that. It was constructed in such a way that it suggested or requested or required that we lay out a science program that would yield the necessary answers to demonstrate ignition burning, and so on, and that simultaneously with that we lay out an engineering program that anticipates those results and would be up and running at such a point as the necessary ignition results came, in order to make the reactor by the year 2000. It has not for fiscal reasons been possible to sustain that kind of a program. So, it has been more in the fiscal area than in the technical or programmatic area that that has had a problem.

Mr. **Morrison**. So, we share the burden of that slippage, because it certainly has existed.

Part of the deliberation that I have heard revolves around the question of whether we should be developing small or large fusion machinery. What is your view of this debate?

Dr. **Trivelpiece**. The scaling laws for toroidal devices permit you to either at a constant, low plasma density, make them large and achieve the necessary insulation to get burn and ignition. Or you can at higher magnetic fields and smaller physical volume accomplish essentially the same region and parameter space.

In one case you encounter certain problems of large physical size and in the other case you encounter some problems having to do with the strength of materials related to the ability to create the magnetic fields. Both processes are worth understanding in the limits that they impose and the opportunities that they provide.

The laws of nature are at work here, though, in such a way that you cannot arbitrarily say that I would like one that is no bigger than a breadbox and then have the laws of nature work to your advantage to make that possible. It does require some obedience to the laws of physics that are at work here. And to the extent that they are both science and technology limitations, I think they should be explored to find out what they are in order to have the most effective and practical system.

I don't think it is a matter of simply the desirability of one versus the other, but it is the practicality. The answers to that should be obtained along the way.

Mr. **Morrison**. If the Department continues to spend about $500 million a year for the next 20 years, when do you anticipate that we will be ready for commercialization of fusion power?

Dr. **Trivelpiece**. The $500 million a year times 20 years' number keeps coming up from time to time. I submit that I doubt that we would spend it for one of either two reasons. The first reason would be that we discover a fundamental show stopper, and it says that you are not going to ever make it with fusion, in which case you wouldn't spent that kind of money for 20 years. The other is that it succeeds in such a way that it becomes clear that technically that is possible to do and that it can be transferred over into a commercial activity in an appropriate manner, in which case then the burden of some of the longer term engineering costs and development and commercialization would be borne through the private sector.

So, once again, I don't think you would do it at $500 million a year for 20 years. So, there is a finite envelope. The envelope is terminated either by success or by failure. At the moment, I would prefer to believe that it will be terminated by success and that at some point the benefits of this will be sufficiently obvious that the kinds of devices that are implicit in the Fusion Program can come into existence and take their place in the commercial world.

Mr. **Morrison**. Thank you. I guess I don't mind if we terminate the program through success or failure. I just don't want to terminate it through default.

Dr. **Trivelpiece**. I would agree with that.

Mr. **Morrison**. Thank you.

Madam Chairman, thank you for this opportunity.

Mrs. **Lloyd**. Thank you very much, Mr. Morrison.

Following up on the discussion on the large versus the small, because in the near term, ignition is certainly a major goal in the program. We do need a significant experiment to verify the quality and the sustainability of a heat producing plasma.

The administration, of course, is pushing for international cooperation for a larger device. I think we have to realize it is going to take 3 to 4 years to formulate such a cooperative experiment. It will probably take 6 to 9 years to build and over $1 billion. This leaves the United States with a challenge of adopting a near-term program and a low-cost ignition device. We are talking about something under $500 million, so that we can go forward.

If we are to cooperate with other nations on the fabrication and operation of a large ignition device, what would be the major parameters of such a device? What would be the primary features or components of the large device? What would be the advantage over building the larger ignition device versus the low cost, assuming that it can be built at least 5 years later than the low-cost burner?

In other words, we need to be doing something before the 7 or 8 years that it will take to build the large.

Dr. TRIVELPIECE. I am not sure that the administration is pushing in the international arena for a jointly constructed large device. What I was trying to characterize that we are doing through the Versailles process is to identify a program so that whatever ends up happening does not result in unnecessary duplication of expensive facilities, so that there is a cooperative arrangement around the world.

If the science and physics were to suggest that a large next step were appropriate, then how would we cooperate with the other countries interested in doing this in a way to make it or perhaps more than one facility available and have the information generated? The idea of a lower cost device in the United States to work toward ignition is very attractive. Obviously, in the present circumstance it would have to be done probably within the fixed financial envelope that we are dealing with.

I think the right approach would be to generate the necessary information to come to a rational conclusion as to what kind of a next step device might be appropriate for the United States. We have devoted some effort to that, seeking the answer to that question.

Mrs. LLOYD. But in the meantime, we need to get on with our act.

Dr. TRIVELPIECE. Yes.

Mrs. LLOYD. Wouldn't a lower cost device be appropriate?

Dr. TRIVELPIECE. I guess the answer is yes, but I think I would like to see that answer emerge as a result of a technical study that had that as its conclusion rather than making an ex catheda judgment on it.

Mrs. LLOYD. Dr. Trivelpiece, assuming that a successful, low-cost ignition device is built and is tested, what do you think the next step would be on the path toward the peaceful application of fusion energy? Would the device differ, a perhaps one-half billion dollar device that would be built 5 years later than the lower cost device?

Dr. TRIVELPIECE. The object of the Fusion Program is to produce a copious supply of neutrons from the fusion process by creating a plasma at an appropriate temperature and density and hanging on to it for a long enough period of time to have the investment repaid. That is essentially the definition of burning and ignition. Whether that is in a large device or a small device I don't think is as important as it is that it occurs in some device. If it turns out that the practicalities are such that a large device can be a useful means of generating fusion neutrons for practical application, then that would be the course of events that would be best to follow. If it turns out that it is possible in a smaller device to produce the same results and do so in a shorter time frame and at a lower net cost, yes, obviously we should do that.

I am having trouble, I think, sensing exactly what it is that you would like me to come to a conclusion on because I think we agree on the subject, that, yes, such a thing would be valuable to do. And, yes, it would be desirable to get on with it. How would we do it, however, in the present financial constraints under which we are operating and undo some of the other things that are also generating useful information toward the eventual goal?

So, it is a balance problem. We try to maintain a balanced collection of activities here that supply the information in various segments that are needed, all simultaneously. If you stop all that to do something else, that may not be the most productive thing to do. So, to examine the feasibility and desirability of an experiment such as you suggest in the context of the program is an important thing to do.

I would certainly take that as a mission to try to develop that information and present to the committee at some time in the future.

Mrs. LLOYD. What kind of an answer can we have on this in the 1987 budget? Are we going for international cooperation? Are we going to stop along the way and do something that we can afford to do in the near term? I think this has a lot to do with our program. I think that this is something that we should define in this year's budget.

Dr. TRIVELPIECE. The 1987 budget or the——

Mrs. LLOYD. I want to know what your plans are for the 1987 budget as well. I think the best thing that this program needs right now is a goal.

Dr. TRIVELPIECE. I hope it doesn't seem irresponsible, but we have not given that much thought yet to the 1987 budget process and the planning therefor. The Department is only in the most preliminary stages of 1987 budget preparation. I have tended to focus my interests on the 1986 budget at the moment and how to plan for it. It will be happening before too long.

Mrs. LLOYD. I don't mean to keep dwelling on it, but I think that we do determine what the Department's goals are. Are we working toward international cooperation, which is going to take a long time? Or perhaps could we build a small device in cooperation with the Japanese?

Dr. TRIVELPIECE. International cooperation is not a substitute for a sound U.S. program. I think the essential feature is that we have a sound U.S. Fusion Program. The fact that other countries are engaged in this research is something that we can take advantage of in a cooperative and collaborative way. To the extent that that makes sense, we should do so. We shouldn't put ourselves, I believe, in a position where we are critically dependent for results in the United States on activities that are going to occur overseas.

The other side of that is that it would seem foolish for us to simply duplicate for the purpose of duplication activities that have occurred overseas.

So, it is not so much a substitute but, rather, a cooperative and collaborative program in which the whole is greater than the sum of the parts.

Mrs. LLOYD. What has been the Japanese reaction to the termination of FMIT activity in the United States?

Dr. TRIVELPIECE. I am not sure how to characterize it, but I don't think they were pleased.

Mrs. **Lloyd**. Don't you think that the diplomatic impacts of SRC2 and the FMIT terminations are much more serious for our partners than they would have been if we had been the affected party?

Dr. **Trivelpiece**. I believe that it is important for the United States, once it enters into international agreements and negotiations, that it should maintain its word in trying to do this. This is done sometimes with difficulty, and we have maintained a large coil test project at Oak Ridge, even in the face of budget reductions. It is in part because we feel just that kind of an obligation to live up to our word.

We had not concluded the agreements with the Japanese on the FMIT. But I think the disappointment is nevertheless there. And it would have been desirable to have been able to continue that project.

Mrs. **Lloyd**. I think that our international credibility has been eroding on a rapid downhill path for the past 6 years. I think you will have to agree with that, too, Mr. Carter's policy down to OMB's policy of not keeping their word. I don't think it's a major factor with OMB any more than it was with the Carter administration.

I think to regain our international credibility should be one of the major goals of not only this committee but your Department as well, Dr. Trivelpiece.

Is DT scientific feasibility experiment at the Princeton TFTR on the critical path for subsequent ignition experiments?

Dr. **Trivelpiece**. I believe that it is. I believe that it is essential that we go through the demonstration process of establishing the plasma conditions in hydrogen and then establishing the same conditions in the deuterium-tritium plasma and determining once and for all that you can indeed release significant amounts of energy through the fusion process in a controlled way.

Where we would go from there I am not quite so clear on.

Mrs. **Lloyd**. Does the slippage in the program have a corresponding delay at the time that we will have achieved the capability to execute an ignition experiment? Is there a parallel there?

Dr. **Trivelpiece**. I believe that it must essentially generate some delay. I would hesitate to say how much. Perhaps Dr. Clarke would care to amplify on that.

Dr. **Clarke**. Yes, there certainly has been a delay. The purpose of the present TFTR Program as we conceive it is to use deuterium alone to establish those scientific conditions under which ignition can take place. That will give us critical information as to how to design the ignition experiment that we have been discussing.

What has suffered is the experience of dealing with deuterium and tritium. As Dr. Trivelpiece indicated, we have had to postpone the provision of equipment for those experiments.

It is a little bit difficult to say what ultimate delay in ignition capability would result from that. Until the scientific experiments in deuterium are done, it is a little bit difficult to say how important the followon tritium experiments in TFTR will be. We will have to learn the importance of that tritium experiment on the basis of the data from the first stage. But the inevitable delay of the equipment for DT operation is a fact. We cannot avoid that.

Mrs. **LLOYD**. What is the Department of Energy's involvement in the plasma heating toroidal experiments?

Dr. **TRIVELPIECE**. Oh, the OTHE experiment of GA Technologies?

Mrs. **LLOYD**. Yes.

Dr. **TRIVELPIECE**. There are probably certain aspects and negotiations that I am not really at liberty to discuss because it is in the process of being investigated with some commercial companies. But the general process is that we are reviewing the prospects of doing that and forming an arrangement by which some progress could be made.

I am reluctant to reveal too much in a way because it does involve some commercial activities with a company and some proposals that are under current review.

Mrs. **LLOYD**. All right, we will talk about that then at a later time.

Would you comment on the potential for such toroidal devices as reversed field pinches, toroid for overtaking the Tokamak and becoming attractive as possible fusion reactors?

Dr. **TRIVELPIECE**. I guess I am pessimistic that they would overtake it, in the sense that the Tokamak approach clearly has the lead position at the moment. There has been a tremendous investment made and making progress. From a scientific point of view, it is important to generate the information that these other devices are capable of producing. But it would be perhaps a bit unwarranted to conclude that with a little effort they could easily overtake what has been the clear frontrunner in terms of making progress toward ignition and burn.

Mrs. **LLOYD**. You are very well aware of the fact that there are individuals that believe that fusion energy is going to have to compete with coal and nuclear energy on a cost-competitive basis and that the costs of the large fusion devices are very high. So, I think that this is certainly something that has to be a factor that we must consider as well.

I thank you very much for your testimony today. Thank you for being here. I am sorry there was a delay in starting our hearings.

We will have some additional questions from members of the committee that could not be here today. Thank you very much.

Dr. **TRIVELPIECE**. Thank you, Madam Chairman.

Mrs. **LLOYD**. Our next witness is Hon. Mike McCormack, who formerly chaired this committee so well. His service to this committee is still an overriding feature because he indeed was the author of the Fusion Engineering Act. So, welcome, Mike. We would like to hear from you, as we like to every year on the Fusion Program. You may proceed as you wish.

STATEMENT OF HON. MIKE McCORMACK, FORMER U.S. REPRESENTATIVE

Mr. **McCORMACK**. Thank you, Madam Chairman, Mr. Morrison and Mr. Stallings.

If I may go off the record for a moment——

[Off the record.]

Mr. **McCormack**. I am delighted to have this opportunity to appear again before the subcommittee and discuss some thoughts concerning this Nation's Fusion Program with you.

I want to thank you for your consideration in inviting me to testify, Madam Chairman, as I am sure you are aware it was absolutely impossible for me to make the rescheduled date for my testimony. I appreciate it.

Mrs. **Lloyd**. We are glad to have you with us any day, Mike.

Mr. **McCormack**. I would like to establish that I am speaking for myself alone.

A year ago when I was invited to speak before the committee I was consulting with INESCO. But as you are aware, INESCO, lacking adequate funding to continue, has discontinued its operations. I speak entirely for myself today.

I come before the committee to propose that you request a study of a new initiative in magnetic fusion research. I base this recommendation on several assumptions which I believe to be realistic even though they may be only reluctantly accepted by some.

First, there is little likelihood of Federal funding and certainly less of private funding for a new large next step magnetic fusion experiment within the foreseeable future in the United States.

Such a large next step, Madam Chairman, as envisioned in recent consideration of the TFCX, would be a Tokamak somewhat larger than TFTR and capable of limited DT burns. Estimates of the cost of this range from $1.5 to $2 billion. As far as I know, and I am pretty sure this is correct, this does not include any serious consideration of a provision for realistic remotization of the facility or the handling of many pieces of equipment that would become radioactive from exposure to neutrons.

Such provisions would probably double the cost of the machine. Such a machine should not be considered as a realistic prospect for the future in this country especially considering budget constraints and administration philosophy for long-range commitments.

Two, the likelihood of meaningful international cooperation to construct such a machine is, I believe, quite low. I don't argue that it shouldn't be pursued, but I suggest that such a path will be fraught with endless difficulty, frustration and delay. I don't consider it realistic for the United States to place its dependence for the future of fusion research and development on international cooperative funding.

Three, it is highly unlikely that a large machine would be a realistic step toward practical commercial application. Today we are witnessing a strong tide of opinion favoring small fission machines. Utility executives tell me that they would be even more reluctant to invest very large sums of money in large fusion machines than they are today in large fission plants.

Four, from all this I conclude, and I think other thoughtful observers have concluded, that the United States should not continue large machine programs beyond the scheduled operation of the TFTR.

Yet we have a mission that we must contrive to accomplish—and I quote from your report of your subcommittee issued a year ago. "The goal of the magnetic fusion program is to develop and ultimately demonstrate the technology for providing electric power from fusion reactors at commercial scale."

Now, if we are to reach this goal, it appears we must do it through the development of small machines. It is now obvious that small fusion machines will be much less expensive per kilowatt hour than large ones. Perhaps they can be made inexpensive and practical enough to be attractive for commercial use.

Five, a great deal more is understood now about plasmas in small machines than was assumed likely even 2 or 3 years ago. This was one benefit I obtained from serving on the science advisory committee for INESCO. Accordingly, there is today a significant body of opinion within the plasma physics community that small machines could be made to work even though they may differ radically in design from each other and from large machines, and may put much higher demands on materials.

Many of the apparent obstacles to success with small machines of several years ago have been substantially overcome. From my private conversations with some of the leading plasma physicists in this country, it is my conclusion that small machines can be built that have a reasonable chance of producing plasma conditions required for sustaining an actual fusion reaction.

Not only would such small machines have the advantage of being much less expensive, but it is also probable they could be simply replaced when, through their operation, they become unusable.

It is quite conceivable that such machines can simply be removed from an operating cell, stored in a shielded cell and allowed to cool for several years, and a new machine installed. These might be zero-maintenance machines, much cheaper, quite like light bulbs, used until they are worn out and then replaced.

The estimates of the cost of such machines vary from $5 to $10 million each. That is a tiny fraction of the more complicated big machine.

Small machine designs include very small Tokamaks such as the Riggatron, the OTHE of GA Technologies technology and reverse field pinch or field reversed configuration, spheromacs and certainly others.

Six, there is considerable interest in designing such small machines within private industry. Mr. John Landis, executive vice president of Stone & Webster and a member of your fusion advisory panel, Madam Chairman, recently commented on this interest and a greatly increased industrial involvement in remarks delivered at the annual meeting of the fusion power associates. Mr. Landis suggested a vigorous initiative by the DOE to involve private industry in magnetic fusion research and development and suggested that DOE make all relevant information on fusion research available without cost to industry.

He believes that this will lead to industrial involvement and the creative innovation that industrial competition always brings.

I applaud John's suggestion but one major obstacle stands in the way. That is realization on the part of every corporation that even if such a machine that they devise would achieve Lawson criteria, the next step would require a site that could handle a fusion burn.

Seven, such a site would not be on a college campus or in a conventional industrial laboratory. It would of necessity be located at a DOE facility with all the preexisting services that major DOE facilities provide: remoteness, security, NRC and DOE supervision, tritium handling, hot lab examination facilities, irradiation monitoring and control, waste disposal facilities, adequate and hopefully low-cost electric power, and scientific and engineering support from experienced on-site contractors.

Accordingly, Madam Chairman, I respectfully suggest that the DOE be requested by this committee to conduct a study of the nature, approximate cost, feasibility and other characteristics of a fusion research center to be located at the DOE site with the facilities I have mentioned and for maximum value I hope the result of such a study could be made ready for consideration as part of the fiscal year 1987 budget.

I should observe that INESCO projected that such a site with two test bays hot storage facilities, remote handling equipment, a very large motor generator, electric conversion systems and electric controls, tritium handling facilities and the most modern diagnostic systems could be built for $150 to $200 million within less than 5 years.

Included in the INESCO projection was the cost of building and testing five small machines and operating buildings as well.

I provide this data only to suggest a ballpark estimate of the cost of the fusion research center. To put it into context with the total funding available for magnetic fusion research, if we assume an average of only $300 million a year for the next 5 years, this would be about 10 percent of the total. One would conclude that if the basic assumption I make has even a modest degree of validity that the fusion program should be reoriented toward exploring a variety of small machines through private industry, then the cost of a fusion research center such as I suggest does not appear to be unreasonable.

Such a center would be available to any governmental agency such as the DOE on Department of Defense to test small fusion machines without cost, and should be available to private industry to test machines, retaining proprietary information, for only the administrative cost involved. Machines may be developed jointly by DOD or DOE with private industry. They could also be tested.

Now, this is similar to the policy developed by NASA for the use of its wind tunnels by public and private agencies. I see no obstacle to such an arrangement for a fusion research center. It is, in fact, consistant with administration philosophy to encourage private industry to develop promising energy technologies; and it is consistent with the existing DOE philosophy of encouraging private contractors to use nondefense DOE facilities for research.

Madam Chairman, as you are aware, this testimony has been prepared in some haste for me to testify today. Therefore, the testimony may lack some of the sophistication or detail that otherwise would be expected. I trust you understand and you will accept this testimony as a simple attempt to make a constructive suggestion without attempting to go into great detail.

I thank you for your courtesy and consideration in allowing me to testify at this time. Of course, I will try to answer any questions.

[The prepared statement Mr. McCormack follows:]

TESTIMONY BY

HON. MIKE McCORMACK

BEFORE THE SUBCOMMITTEE ON ENERGY
RESEARCH AND PRODUCTION

HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

MARCH 8, 1985

Thank you, Madam Chairman. I am delighted to have this opportunity to appear again before the Subcommittee on Energy Research and Production and to discuss with you some thoughts concerning this nation's program for magnetic fusion research.

Before I continue Madam Chairman, I should like to establish that I am speaking for myself alone; not for any other entity or individual -- public or private. Last year when I was asked to address the subcommittee I was retained as a consultant by Inesco, Inc. the California based company which was developing a small, high density expendable copper magnetic fusion device called the Riggatron. My consulting assignment was in part to assemble and chair the Inesco Science and Technology Study Committee made up of some of the world's outstanding scientists and engineers.

Since that time Inesco -- lacking adequate funding to continue -- has discontinued its operations. I make this point only to establish clearly that I am speaking for myself alone.

Madam Chairman, I come before the Committee today to propose that you request a study of a new initiative in magnetic fusion research. I base this recommendation on several assumptions which I believe to be realistic, even though they may be only reluctantly accepted by some. They are:

1) There is little likelihood of federal funding -- and certainly less of private funding -- for a new large "next-step" magnetic fusion experiment within the foreseeable future in the U.S.. Such a large "next step" machine, as envisioned in recent consideration of a TFCX, would be a tokamak somewhat larger than the TFTR and capable of limited deuterium-tritium burns. Estimates of the costs of such a machine vary from $1-1/2 to $2 billion, but as far as I know, this does not include any serious consideration of -- or provision for -- realistic remotization of the facility, or the handling of the many pieces of equipment that would become radioactive from exposure to neutrons. (Such provisions would probably double the cost of the machine.) Thus, such a machine may not be considered as a realistic prospect for the foreseeable future in this country.

2) The likelihood of meaningful international cooperation to construct such a machine is, I believe, quite low. I do not argue that it should not be pursued, but I suggest that such a path will be fraught with endless difficulty, frustration and delay. I do not consider it realistic for the United States to place its dependence for the future of fusion research and development on international cooperative funding.

3) It is highly unlikely that a large machine would be a realistic step toward practical commercial application. Today we are witnessing a strong tide of opinion favoring smaller fission machines.

Utility executives tell me that they would be even more reluctant to invest very large sums of money in large fusion machines than they are today in large fission plants.

4) From all this, I conclude -- as I think many other thoughtful observers have concluded -- that the U.S. should not continue large machine programs beyond the scheduled operation of the TFTR. Yet we have a mission -- and we must contrive to accomplish it. I quote from the report of your subcommittee, issued one year ago: "The goal of the Magnetic Fusion program is to develop and ultimately demonstrate the technology for providing electric power from fusion reactors at commercial-scale." If we are to reach this goal, it now appears that we must do it through the development of small machines. It is now obvious that small fusion machines will be much less expensive per KWH than large ones. Perhaps they can be made inexpensive enough -- and practical enough -- to be attractive for commercial use.

5) A great deal more is understood now about plasmas in small machines than was assumed likely even two or three years ago. Accordingly, there is today a significant body of opinion within the plasma physics community that small machines could be made to work, even though they may differ radically in design from each other and from large machines, and may put higher demands on materials.

Many of the apparent obstacles to success with small machines of several years ago have been substantially overcome, and from my private conversations with some of the leading plasma physicists in the country, it is my conclusion that small machines can be built that have a reasonable chance of producing plasma conditions required for sustaining an actual fusion reaction.

Not only would such small machines have the advantage of being much less expensive, but it is also probable that they can be simply replaced when, through operation, they become unusable. It is quite conceivable that such machines can simply be removed from an operating cell, stored in a shielded cell and allowed to cool for several years -- and a new machine installed. These might be "zero-maintenance" machines, used -- quite like light-bulbs -- until they are worn out, and then replaced. The estimates of the cost of such machines vary from $5-10 million each, a tiny fraction of the more complicated big machine. Small machine designs include very small tokamaks (such as the Riggatron), the OTHE of GA Technologies, and additional reverse field pinch or field reverse configuration machines, spheromaks, and certainly others.

6) There is considerable interest in designing such machines within private industry. Mr. John Landis, Executive Vice President of Stone & Webster Engineering (and a member of your Fusion Advisory Panel, Madam Chairman), recently commented on this interest -- and a greatly increased industrial involvement -- in remarks delivered at the annual meeting of the Fusion Power Associates in La Jolla on February 1, 1985. Mr. Landis suggested a vigorous initiative to involve private industry in magnetic fusion research and development, and suggested that the DOE make all relevant information on fusion research available -- without cost to industry. He believes that this will lead to industrial involvement and the creative innovation that industrial competition always brings. I applaud Mr. Landis' suggestion, but one major obstacle stands in the way, and that is the realization that even if such a machine would achieve Lawson criteria, the next step would require a research site that could handle a fusion burn.

7) Such a site would not be on a college campus or in a conventional industrial laboratory. It would, of necessity, be located at a DOE facility with all of the pre-existing services that major DOE facilities provide: remoteness, security, NRC and DOE supervision, tritium handling facilities, available hot lab examination facilities, radiation monitoring and control, waste disposal facilities, adequate (low-cost) electric power, and scientific and engineering support from experienced on-site contractors.

Accordingly, Madam Chairman, I respectfully suggest that the DOE be requested to conduct a study of the nature, approximate cost, feasibility, and other primary characteristics of a Fusion Research Center, to be located at a DOE site with the facilities I have mentioned. I should observe that Inesco projected that such a site, with two test bays, hot storage facilities, remote handling equipment, a very large motor generator, electric conversion systems and electronic controls, tritium handling facilities, and the most modern diagnostic systems could be built for $150 to $200 million within less than five years. Included in the Inesco projection was the cost of building and testing five small machines. I provide this data only to suggest a "ballpark" estimate of the cost of a Fusion Research Center, and to put it into context with the total funding available from magnetic fusion research if we assume an average of $300 million a year for the next five years. One would conclude that if the basic assumption I make has even a modest degree of validity: that the fusion program should be reoriented toward exploring a variety of small machines, then the cost of a Fusion Research Center such as I suggest does not appear to be unreasonable.

A Fusion Research Center would be available to any governmental agency, such as the Department of Energy or Department of Defense to test small machines without cost, and should be available to private industry to test machines, retaining proprietary information, for only the added cost involved.

Machines developed jointly by the DOD or DOE with private industry could also be tested. This is similar to the policy developed by the NASA for the use of its wind tunnels by public or private agencies. I see no legal or legislative obstacle to such an arrangement for a Fusion Research Center. It is, in fact, consistent with Administration philosophy to encourage private industry to develop promising energy technologies; and it is consistent with the existing DOE philosophy of encouraging private contractors to use non-defense DOE facilities for research.

Madam Chairman, as you are aware this testimony has been prepared in some haste so that I could testify today (because of a schedule conflict that makes it impossible for me to testify at a subsequent date with other outside witnesses). Therefore, the testimony may lack some of the sophistication or detail that would otherwise be expected. I trust that you will understand and that you will accept this testimony as a simple attempt to make a constructive suggestion without attempting to go into great detail concerning it.

I thank you for your courtesy and consideration, allowing me to testify at this time.

I will, of course, be happy to attempt to answer your questions.

Mrs. **Lloyd**. Thank you, Mike. It is excellent testimony. You couldn't have done any better if you spent three or four days getting ready for it. It is right on point.

You mentioned INESCO's projection of $150 to $200 million for a fusion research center. I was just wondering if you were familiar with Dr. John Sheffield at Oak Ridge. He has a study that concluded that 75 percent of the total costs in the supporting facilities—power supplies, tritium handling and so forth—and only 25 percent of the Tokamak. So do you feel that $200 million is really sufficient to cover the supporting facilities?

Mr. **McCormack**. This was a very seriously considered number prepared by the engineers from INESCO and included in their prospectus for the Securities Exchange Commission. You must understand the machines themselves were projected to cost only from $5 to $10 million, the motor generator between $35 and $50 million.

So that you had then $100 million approximately for the buildings and electronic controls and diagnostics. It is not a big site. This projection may be optimistic. It may be totally different when private industry is looking at it from the DOE looking at it through Federal contracts. I don't think it is unrealistic. With small machines you would have even a larger percentage of the cost in the facility and the operation of it than in the machines themselves.

It could be, I would not be surprised if it would be 85 percent for the facility.

Mrs. **Lloyd**. This is a very intriguing concept. You also mentioned the diagnostic systems would be included. Do you have any information concerning the diagnostics on the existing fusion devices?

Mr. **McCormack**. No, Madam Chairman. Unfortunately, that is part of the lack of sophistication of this statement; that is not there. I must beg off on that point.

I simply am not familiar enough with the diagnostic systems to comment on them intelligently.

Mrs. **Lloyd**. As I was reading through your testimony, as you were presenting it, there was one thought that kept coming back to my mind. That is could you suggest any existing facilities that would be executable for such a fusion research center? I kept thinking I was going to come across it in the next paragraph.

Mr. **McCormack**. Going to come across a site?

Mrs. **Lloyd**. I thought you might suggest an existing site for the facility.

Mr. **McCormack**. I think you could use any one of the existing DOE facilities that have these characteristics. I think Oak Ridge would qualify, perhaps even Los Alamos, certainly Idaho and certainly Hanford. You say this to get me to Hanford. That is why I included parenthetically low-cost power in that statement about criteria. Power is a very expensive—a very meaningful consideration. It could be a significant part of the operational cost, and when we did our studies for INESCO, Hanford came out way, way, way ahead at that point.

However, I don't want to go too far on that.

Mrs. **Lloyd**. With that comment, I would defer to my colleague.

Mr. Morrison. I can't think of a better introduction, Madam Chairman.

Mike, this is indeed intriguing and again I commend you for the leadership you have evidenced through the years in this particular area.

Just to make the record clear, it is not your intent that the dollars that would go into such a testing facility would be to replace any of the work that is ongoing within the Department of Energy development budgets?

Mr. McCormack. Not necessarily, no. I leave this to you and the Department and the OMB if there is a consideration to develop such a plant.

I only suggest that my estimate of the cost would be probably no more than 10 percent of the fusion budget over the next 5 years. It would take a while to get it going. As you realize, the construction phase of the Fusion Program is going down and moving into operational phases of the equipment that has been built. I do not anticipate that this will be an overly burdensome thing.

Mr. Morrison. Your proposal includes turning over of research results and data to private enterprise. That is not intended just so, say, here we have tried it, you finish it. It is that the sharing would in fact give some incentive for private enterprise to pick up and go on with their own machinery.

Mr. McCormack. By all means. I think this is critically important. I think the point that John Landis made is extremely well taken. There is a lot of interest out there just as there was at one time in the Fission Program. But there is a reluctance to get into fusion, because a lack of understanding and DOE would have to extend itself to private industry as they did before and say here are the facts, here is the information, here is the data, here are the results of our research. And that is all public information for the public good.

Mr. Morrison. Your testimony also indicated you have had contact with a number of people that expressed an interest in this and that this would be perhaps a stimulus that would lead to significant private investment in the fusion arena.

If such a facility had existed would INESCO still be involved in pursuing the Riggatron?

Mr. McCormack. If such a facility had existed I think INESCO would still be in existence, yes. This was clearly a major obstacle, and I cited this as a classic example. INESCO clearly had the money to design and build several Riggatrons. They didn't have the money to go any further, and they didn't know how to get it. How does a small private company come by $150 to $200 million?

The only way you do that is through the Federal Government. Even a large corporation, a large engineering firm, I understand there is a coalition of a number, I don't think I am at liberty to say, but I won't say which ones, a number of the very large engineering companies are now exploring the design of a moderately small fusion device. This is not as far as I know visible anywhere in the DOE program.

This is outside the OTHE, of course. I am sure they will run into the same situation everybody else did. They can build a machine but what do they do with it once they think it is ready to test? In the first place, they will produce a large number of neutrons, and this involves all the problems of radiation control and monitoring, NRC control, waste handling, hot laboratories and everything else. They have to go to such a site, and they have to have a motor generator. As I pointed out, the motor generator for some of these machines costs from $35 to $50 million. It has to be where you are doing your work. If you are going to create radioactive material the equipment becomes radioactive from the neutrons and you should be on a DOE site where you have monitoring and control and waste facilities.

Therefore, it is virtually axiomatic that you must build a motor generator on a DOE facility. This doesn't exist and this doesn't inhibit anyone, I believe, no matter how big they are, from going ahead and trying to devise or get seriously into the business of fusion development.

Mr. **Morrison**. From your experience would you see this as an attraction to the international cooperation that we have been very concerned with here this morning?

Mr. **McCormack**. It is not completely outside the concept of international cooperation. There is some talk of international cooperation in building a machine simply to get a burn and to do it in Canada. There is a Canadian facility—they have offered a facility in Canada that I don't know much about, but I understand it has tritium handling facilities and remote facilities. I don't know how well it qualifies.

There might be international cooperation there. It might be possible for the United States to offer research—use of a research facility on time available to foreign developers as well as domestic developers for appropriate compensation.

Mr. **Morrison**. Or perhaps through a bilateral arrangement with the U.S. company. We see that more and more often. They seem to move much faster than our Governments do as far as working together.

Mr. **McCormack**. Yes. I only want to say that I feel that we should not be dependent upon any international cooperation in the area of funding for the success of our program.

Mr. **Morrison**. We have learned that the hard way.

Thank you, Madam Chairman. I would only comment that I think this suggestion for a study reflects a position of leadership. We should respond in any way we can and perhaps ask Mr. McCormack if the pieces go together if it is the wish of the subcommittee and the committee to proceed with this if he would be available for advice and counsel as we try to fill in some of the blank spaces.

Mr. **McCormack**. I will, of course, try to be of any assistance I can.

Mrs. **Lloyd**. I think we do try to promote international cooperation, but there is no reason we can't proceed on our own as well.

We thank you, Mike.

Mr. Stallings, I must apologize to you. I didn't see you come in when our Government witnesses were here. If you would like to submit questions for the record for them, certainly we would hope that you will. If you have any questions now, please proceed.

Mr. **Stallings**. Yes, I would like to do that. It is nice to see you, Mr. McCormack. I appreciate the efforts you have made in bringing some of us freshmen up to speed on what is going on with the scientific community.

You mentioned Idaho has a possible site for this. Has INESCO done much looking into various sites and how does the INL fit the criteria that you have outlined?

Mr. McCormack. I think it is quite high. I think that frankly Hanford and INL would be major competitors for such a site.

Mrs. Lloyd. We may have to build three of these.

Mr. Morrison. Idaho should be on the list long enough to get through the Senate.

Mr. McCormack. I want to suggest that three of them would be less than our contribution, our portion of any international cooperative venture for a big machine.

Mrs. Lloyd. As I said, there is a lot of validity to your proposal.

Mr. Stallings. You certainly struck the right chords here.

How soon do you feel plasma conditions could be achieved in a compact hearth?

Mr. McCormack. I believe that if we were to initiate a program to build a site, a research facility, and it were on line in, say, 5 or 6 years, that there would be several designs of private machines including the OTHE that would be ready for testing with tritium at that time. So that we could then envision burn tests—assuming everything went smoothly—within 7 or 8 years.

I know that sounds optimistic, but I don't really believe it is. If industry knew that a site would be available, I believe we would have several designs under way right away. I am convinced there is enough known now about the scaling laws and plasma physics that these tests could be conducted with hydrogen in a number of private industrial laboratories and universities and various other places. They could bring the machines—they might be ready to move to a test site for tritium burn within 7 years of something like that.

Mr. Stallings. Thank you.

Mrs. Lloyd. Thank you very much, Michael. We hope to hear from you later on in the year.

Due to the recess through next week, the subcommittee will not meet again until Monday, March 18, when we will hear from our nongovernment witnesses and the advisory panel on the Magnetic Fusion Energy Program.

The hearing will be held in this room, 2318, and will begin at 2:30 p.m.

If there are no further comments, the subcommittee stands adjourned. Thank you.

[Whereupon, at 9:40 a.m., the subcommittee was adjourned, to reconvene Monday, March 18, 1985, at 2:30 p.m., in the same room.]

1986 DEPARTMENT OF ENERGY AUTHORIZATION

MONDAY, MARCH 18, 1985

House of Representatives,
Committee on Science and Technology,
Subcommittee on Energy Research and Production,
Washington, DC.

The subcommittee met, pursuant to notice, at 2:35 p.m., in room 2318, Rayburn House Office Building, Hon. Marilyn Lloyd (chairwoman of the subcommittee) presiding.

Present: Representatives Lloyd, Stallings, Lujan, and Fawell.

Staff present: Dr. John V. Dugan, staff director; Ronald E. Williams, technical consultant; Tom Weimer, Republican energy coordinator; and Tim Peckinpaugh, Republican technical consultant.

Mrs. Lloyd. The Subcommittee on Energy Research and Production will come to order.

Good afternoon everyone. I do ask unanimous consent that the subcommittee permit today's proceedings to be recorded and covered by the media and other persons. Without objection, so ordered.

The Energy Research and Production Subcommittee will receive testimony today from outside witnesses on the Department of Energy's fiscal year 1986 budget request for the Magnetic Fusion Energy Program. From my perspective, I shall repeat the three major issues to be considered in today's hearing. The first is: having suffered reductions of nearly 20 percent over the last 2 years; can the DOE pursue a strong Magnetic Fusion Energy Program without making some drastic changes to reorient it? We shall pursue the related question of whether some additional funding might provide the Department with critical programmatic flexibility.

Although all three major issues are affected by the budget levels, the remaining two issues are inextricably linked to one another. These issues are: the prospects for improving the MFE Program focus through an ignition experiment and the overall question of international cooperation.

The first panel, including the chairman of the subcommittee's advisory group, Mr. Reichle, will address the merits of an ignition experiment. Then our good friend, Mr. Gavin, will provide a most informed and balanced perspective on international cooperation.

It appears most important that the MFE Program have a focus for development and technology beyond TFTR and other operating experimental devices. The community is virtually unanimous that this should be an ignition device, for example, one which produces a burning plasma. We must understand whether it makes programmatic sense for that to be a lower cost device, under $500 million, or should the DOE go for a much more ambitious project? I, for one, believe that the domestic program could afford a more modest ignition device within projected budgets, whereas larger projects—closer to test reactors—can clearly only go forward with significant international cost-sharing.

Clearly, the ignition choices drive the international cooperation approach since Mr. Gavin's National Academy Panel says it will take 3 to 5 years to arrive at a multinational agreement to go forward with a large project. Thus, it appears to me that while the United States should welcome international cost sharing for a low-cost burner project, it should not be a requirement for a go-ahead in the near future. I am very interested in what certain witnesses may have to say about these matters this afternoon.

Before the first panel begins, I ask unanimous consent that Mr. Morrison's statement and statements by Dr. Pete Rose of math sciences, the University Fusion Association and the Atomic Industrial Forum be included in the record.

[The prepared statements of Congressman Morrison, Dr. Kershkowitz, Mr. Rose, and Mr. Matson follows:]

OPENING STATEMENT OF THE HONORABLE SID MORRISON
ON THE FY 1986 DOE AUTHORIZATION REQUEST
FOR MAGNETIC FUSION PROGRAMS

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
MARCH 18, 1985

GOOD AFTERNOON. TODAY WE CONCLUDE OUR DEPARTMENT OF ENERGY AUTHORIZATION HEARINGS ON THE DOE MAGNETIC FUSION ENERGY PROGRAM. WE LEARNED AT THE MARCH 8TH HEARING THAT DOE IS TRYING TO DEFINE AN EVOLVING YET CLEAR FUSION ENERGY DEVELOPMENT MISSION WITHIN THE CONFINES OF A SHRINKING BUDGET. THIS AFTERNOON WITH THE HELP OF THE FINE COMPLEMENT OF WITNESSES BEFORE US, WE NEED TO DISCERN WHAT THE FUTURE FUSION R&D MISSION SHOULD BE.

IT IS CRITICAL THAT OUR COUNTRY MAINTAIN AN AGRESSIVE AND COORDINATED R&D PROGRAM TO PROD FURTHER INTERNATIONAL COOPERATION IN THE FUSION ENERGY ARENA. IN ADDITION, WE NEED TO SCRUTINIZE THE CHANGE IN THE DEPARTMENT'S EMPHASIS AWAY FROM DEMONSTRATION APPLICATIONS TO SMALL-SCALE R&D ON FUNDAMENTAL FUSION CONCEPTS.

MADAM CHAIRMAN, WITH YOUR STRONG LEADERSHIP ON FUSION ENERGY, I'M SURE THAT WE CAN FASHION A PRUDENT PROGRAM THAT ALLOWS US TO INTELLIGENTLY PURSUE DEVELOPMENT OF THIS ESSENTIALLY LIMITLESS SOURCE OF ENERGY AND DO SO WITHIN A CLIMATE OF FISCAL RESTRAINT.

THANK YOU MADAM CHAIRMAN.

UNIVERSITY FUSION ASSOCIATION

OFFICE OF THE PRESIDENT
Noah Hershkowitz

PLEASE REPLY TO
Department of Nuclear Engineering
205 Engineering Research Building
University of Wisconsin
Madison, WI 53706
(608) 263-4970

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University of Toronto
University of Washington
University of Wisconsin
Washington University (St. Louis)

UFA TESTIMONY

I am writing as the President of the University Fusion Association to submit written testimony to the Subcommittee on Energy Research and Production of the House Committee on Science and Technology.

In general we note that the university fusion programs have not taken cuts any worse than average in FY'85 and in the proposed FY'86 budget. However we are concerned that any further cuts in the overall budget would result in substantial cuts to university funding (proportionally much higher than national lab cuts) and also would have a demoralizing effect on the national program. We are also concerned that basic plasma physics and basic engineering science are no longer being supported.

University programs range from large national lab-like programs such as those at MIT to much smaller programs, some as small as one faculty member plus several graduate students (the traditional model supported by the NSF). The bulk of university funding (measured by number of universities) is from APP although Confinement Systems supports several major experiments and D&T supports a limited number of engineering studies. TEXT is the major university experiment supported by APP. Overall, DOE support now involves many groups with many graduate students, several post doctoral students as well as several full time scientists rather than the smaller NSF model.

Much University funding has held up along with APP funding but funding in D&T has suffered some problems. Nevertheless we believe that DOE has demonstrated that they recognize the value of university programs. We believe that the DOE should be commended for several specific actions. These are:

1. Maintaining university support at a time when national labs have had to make significant cuts. In some cases it appears that national labs may have made sacrifices in order to provide university funding and they should be commended as well. However it should be noted that several university programs will probably be sacrificed before the end of FY'86 under the current proposed budget.
2. Introduction of the Magnetic Fusion Science Fellowship which will be instituted this fall to attract the best students into the fusion program. This new fellowship will come from APP and Confinement Systems funds. The Magnetic Fusion Technology Fellowship has also been continued.
3. The policy of encouraging innovative new research and starting new initiatives at universities. We believe that innovative new research in plasma and engineering science is what we do best at universities.

Near level support has not meant that the research support has been frozen in place and this is as it should be. Some groups have received increased support to continue to be relevant and new initiatives require that new money be provided. Clearly, near level support means that some groups must be phased out even though they are making contributions to research.

Unfortunately, APP has decided to phase out small non critical size groups. Critical appears to be defined in terms of size of operation and immediate relevance to the program. It appears to us that this new policy may be contrary to the policy of encouraging innovative ideas because such small groups are likely to be a very cost effective way to generate innovative new ideas. An official policy of only supporting productive efforts, regardless of their size would be preferable.

A continuing problem has recently intensified At the present time, DOE does not support "basic" research related to fusion. Basic non-fusion plasma research is supported to some extent by the NSF. Often there is a fine line between fusion and non-fusion applications. In fact, almost any basic plasma work may have fusion applications. In the past, the Engineering division of the NSF has supported work in plasma science which had applications in fusion or equally valid applications in other areas such as plasma processing, wave propagation, etc (though it has not supported engineering research directed only toward fusion issues). It appears that the NSF Engineering division may now be taking a much more narrow point of view and may not be supporting any plasma research that has any possible fusion application -- which in fact includes almost all basic plasma research. This means that at the present time there is no support for basic plasma-fusion research. This will have a particularly big effect on small "non critical" university programs and will result in a substantial reduction in new innovative idea generation in the U.S. The Japanese on the other hand continue to support basic plasma and engineering research and to generate new ideas. It is likely that the policy of not supporting basic research will drive promising students into other areas of work at universities so that when the few remaining groups involved in such efforts disappear, they will not be replaced. Basic research has been an important part of the background of many of the current generation of fusion researchers. Smaller scale experiments provide students with an opportunity to make mistakes that are not possible on large scale fusion experiments because such mistakes could be quite costly. Small scale investigations by both theorists and experimentalists can be carried out on a timescale that is less determined by milestones but may be more conducive to the training of scientists and engineers. By cutting off basic research we are likely to be eliminating many future leaders of the program.

The official DOE policy with respect to plasma diagnostics is that small groups must merge with larger efforts or disappear. University association with national labs often comes about by way of providing innovative new diagnostics, e.g. heavy beam probes now used to determine plasma potential in many fusion-studies devices. In the past the DOE directly supported both diagnostic development and operation by university groups. Now only development is supported although labs can still employ universities to provide the diagnostics. At a time of decreasing budgets this support is likely to be less available. Of particular concern is the statement in the FY'86 presidential budget that diagnostic support will be "at a reduced level".

Although universities are only a small part of the fusion program from the standpoint of dollars, we make a much larger contribution to the science part of the program. While the national labs also make a significant contribution to science, the work they carry out is much more clearly "science with a purpose". University research while still under strong performance pressure tends to be more basic and less milestone oriented. While milestones are useful to establish goals, the most important results of university programs have often been those that were not predicted in milestones.

Universities provide the manpower on which the program is built and also are a source of innovative new ideas. In addition, larger university programs make a substantial contribution to the fusion data base. Universities should be regarded as a national resource.

Universities have a special need because the timescale of graduate student education is typically 5 years or more with 3-4 years spent actively involved in research. Research assistantships must be provided or students will certainly go off to work in other research areas. This is particularly important for support for engineering students who can readily obtain high-paying jobs in other areas without any graduate training. Graduate students in their last 3 years as students do function in many ways at the level of scientists. This labor generally is very cost effective to the program.

Our biggest concern at the present time is that if further cuts are made in the FY'86 budget or if cuts are made in the FY'87 budget, very substantial cuts (much greater than average) will come from universities. It is likely that the national labs have sacrificed as much as they can or are willing to. Since the total support of university programs (excluding MIT) amounts to no more than $20M, relatively little could be saved by taking even the extreme step of eliminating all university programs.

Noah Hershkowitz

Noah Hershkowitz
Professor of Nuclear Engineering
University of Wisconsin-Madison

TESTIMONY IN SUPPORT OF
DOE MAGNETIC FUSION PROGRAM

by Peter H. Rose, President
Spectra Technology, Inc.
(formerly Mathematical Sciences Northwest, Inc.)

I'd like to speak on behalf of the Energy Department's proposal to initiate construction of a new Field Reversed Configuration (FRC) Physics Experiment in FY86 as outlined in the budget submittal. This proposed experiment is based on the work performed over the past four years at both my company, Spectra Technology, Inc., formerly (before January 1985) known as Mathematical Sciences Northwest, and at Los Alamos National Laboratory, as well as experiments in Japan and the USSR. Our experiments have been extremely successful and we have demonstrated about as much as can be done with facilities of the current size. This work has received extensive peer review and acceptance by the fusion community. Further, it is the general consensus that this approach to fusion is extremely promising and that the next step in physics knowledge is called for at this time. This recommendation was clearly made by the Magnetic Fusion Advisory Committee Panel IX in the 12 July 1985 report, where they specifically called for the construction of this experiment. The experiment which has been proposed will achieve the very significant plasma condition of $n\tau \sim 10^{13}$ and can be accomplished at a construction budget of less than $10 Mil. This experiment will truly take its place as a world-class fusion experiment.

The FRC, plasma confinement scheme appears to be the fusion concept which is most ideally suited to development in small unit size and commercialization based on multiplexing of small, standardized units. The system is inherently simple, high density and, due to its high β, uses the magnetic field efficiently. Consequently, it has high mass utilization, a critical parameter in fusion plant economics. The reactor power for FRC's can be scaled by simply altering the repetition rate of the process, i.e., duty cycle. The cylindrical geometry allows major design simplifications and flexibility such as a vertical reactor chamber, which is amenable to natural convection cooling in an emergency situation. FRC fusion plants could be built similar to current, inherently safe fission reactor designs such as the GE PRISM proposal. Due to the ability to translate FRC plasmas there are no fixed relationships between the plasma conditions and the wall loading. In fact, the FRC may be the only magnetic confinement concept which could be made so small, that the resulting extreme wall loading would make it necessary to resort to a liquid lithium curtain wall, such as has been investigated for the Inertial Confinement Fusion program. Within our current knowledge of scaling it appears reasonable to design small FRC reactors which can be handled by conventional cooled wall technology or which would require a liquid lithium waterfall. Both concepts can result in a very small size fusion power core.

The details of these FRC reactors have not been defined in detail because such work has not yet been done. What we do know today is that the FRC physics looks good and is on a solid footing. There are scaling issues which can be faced in the next several years in the experiment presently proposed in the DOE program. Novel technological approaches are possible since FRC confinement has many characteristics that are very different from mainline fusion confinement.

I am convinced that the FRC concept can lead to a radically different and very cost effective approach to developing fusion as a viable power option. The proposed new FRC experiment, which we call the large $\overline{s}$ experiment, or LSX, is not a large investment, even in current Applied Plasma Physics program terms. The experiment should produce physics scaling data within a factor of two or three of an ignited, reactor prototype plasma in terms of the critical scale length parameters. Speculating where an FRC program could go beyond such a large $\overline{s}$ experiment, in terms of its plasma physics at least it would be very similar to the requirements of a pilot plant or test reactor. It could be an order of magnitude less costly to achieve this level of development than it is for present mainline approaches.

A "proof by test" program of a single unit from development--through to certification could be contemplated. I would maintain that the cost of this PRC "proof by test", could be very close to the same as the PRISM "proof by test" cost which has been estimated by GE to be about $500 Mil. Such a program would be very attractive to industry and should be economically and politically more feasible and saleable than the current mainline approaches.

Atomic Industrial Forum
Committee on Fusion
Statement by Kenneth L. Matson
Before the Subcommittee on Energy Research and Production
of the Committee on Science and Technology
U.S. House of Representatives
March 18, 1985

Thank you Madam Chairman. I am Kenneth L. Matson, Vice President of PSE&G Research Corporation, a wholly-owned subsidiary of Public Service Electric and Gas Company, and Chairman of the Atomic Industrial Forum's Committee on Fusion. It is as chairman of the AIF Committee that I am appearing before you today.

As you know the Atomic Industrial Forum is an international trade association for organizations that share a dedication to the peaceful use of nuclear energy in the generation of electricity, in medicine, in food preservation, and in industrial development. More than 100 of the AIF's 500 member organizations are outside the United States--in Europe, the Far East and South America. The membership includes electric utilities, architect-engineers, equipment manufacturers, mining and milling companies, laboratories, labor unions, universities, and financial institutions.

Since 1975 the AIF Committee on Fusion has followed the development of fusion energy, keeping our member organizations informed of its progress and advising on what industry's role should be.

Support for the fusion program, in many ways, reflects the national response to the need for energy. Five years ago about 70% of the U.S. public listed energy as one of the major issues confronting the country. Today the same polls show that fewer than 2% of the U.S. public think immediately of energy as a top national concern. Without a clear recognition that reasonably priced oil and gas are rapidly diminishing resources, it is hard to focus on the need for new energy systems in the next century.

Major advances in magnetic fusion development in the 1970's combined with the recognition of energy supply as a significant issue led to overwhelming support for the Magnetic Fusion Engineering Act which was passed by the Congress in 1980. That support has faded along with the public perception of an energy crisis.

This lack of support, even interest in the fusion program, was reflected in the congressional action which reduced the FY 85 magnetic fusion budget by about 15%. I realize that this congressional action was taken over the objections of this Subcommittee.

Coming on the heels of this congressional cut is DOE's proposed FY 86 budget which recommends a further 10% cut in the magnetic fusion program.

Such cuts are serving to shift the emphasis of the magnetic fusion program away from the objective of useful energy production--an objective established in the early 1970's and reaffirmed in the Magnetic Fusion Engineering Act of 1980, back to a scientific research program. This shift is uncoupling program activities from parallel engineering and technology developments needed for useful energy production. It is in this engineering and technology area that industry has the most to offer. Its expertise and experience is in engineering systems to provide competitive energy production.

As the budget cuts serve to uncouple program activities from a clearly defined goal of useful energy production industry interest understandably lessens. We are seeing this result in the commitment of fewer industry resources and less senior management involvement in fusion power development.

As we have stated to this Subcommittee on more than one occasion, we believe that progress in magnetic fusion development is limited not by scientific or technical barriers but by the availability of funds. Increases in program funding could be effectively utilized to realize at a much earlier date the extent of fusion energy's significant potential. In the long term, increased funds today will reduce the total costs tomorrow.

Prior to the current budget crisis, the focus of the magnetic fusion program was a reactor system that could challenge the imagination and interest of the U.S. electric utilities. Unfortunately, the redirected focus brought on by these budget cuts is taking away this challenge.

Further, the reduced and uncertain funding revives the argument of differing program concepts. On the one hand we have the rollback concept in which the goal of a reactor system for useful energy production is designed and the development program structured to meet the reactor systems requirements. On the other hand we have the roll-forward concept in which advances in the science define the outcome.

In enacting the Magnetic Fusion Engineering Act, Congress adopted the rollback concept for the program. The recent budget cuts are serving to change the program's concept to roll forward.

Without a clear and practical program objective, as provided in a rollback concept, it is doubtful that a plasma fusion research program can achieve enough support to warrant the half billion dollar or more annual budget the program needs.

We believe it crucial that the rollback concept is maintained and hope the Congress would provide a level of funding that will sustain a direct goal of useful energy production. Even to reserve the opportunity to accelerate the program at a later date requires that FY 86 funding be restored to FY 84 levels.

We have also noted that as program funding is cut there is a push for international cooperation.

Certainly, international cooperation in fusion development has set a notable standard for development programs. The cooperation has been truly international and has served to benefit every fusion program including our own in the United States.

There are two points I would like to make about international cooperation. The first is that it is not a substitute for adequate funding. International cooperation is most successful when it is based on strong national programs so that there is quid pro quo. The second point is that international cooperation works best in the program's scientific research phase. As soon as the program reaches the stage where commercial advantage can accrue international cooperation becomes difficult. We think it is a mistake to believe that international cooperation can make up for inadequate program funding. This is not to say that the U.S. program should not encourage international cooperation. We have seen the excellent results of Japanese cooperation in General Atomic's Doublet project and we believe that many of the same advantages could apply to international cooperation for the FMIT. However, these examples are based on the strength of the U.S. program. Other countries are only willing to contribute funds to an ongoing successful program.

One final comment, Madam Chairman. If the U.S. backs away from its magnetic fusion program after coming this far in its development, it could have far-reaching consequences of historical significance. It is encumbent on this generation to develop fusion as an energy potential for the next generation. Fusion's promise as a virtually inexhaustible source of clean energy for future generations warrants no less than our sustained best efforts to make it work.

This concludes my prepared statement.

Mrs. LLOYD. Our first witness is Congressman Pete Stark who has been a strong supporter of the Magnetic Fusion Program. Pete, it's good to see you. Your written statement will be included in the hearing record; you may summarize or proceed as you wish.

STATEMENT OF HON. FORTNEY H. (PETE) STARK, MEMBER OF CONGRESS FROM CALIFORNIA, ON BEHALF OF THE FISCAL YEAR 1986 FUSION ENERGY BUDGET AND THE LAWRENCE LIVERMORE NATIONAL LABORATORY

Mr. STARK. Thank you, Madam Chairman. I want to thank you and your subcommittee and the full committee for the work that you have been doing and the leadership that you have offered in this field.

While we all know that there are uncertainties in the future of fusion, until its' disqualification as a clean and safe source of energy has been established, it would seem to me that the better part of valor would be for us to continue our exploration into this frontier.

I'm here today just to ask for two things. First, not to make any deeper cuts than the President has already suggested. I'm sure we've all had people coming before a variety of committees in this Congress asking to reinstate dollars. I have a hunch that may be wasted effort on my part, but I do suggest that as long as the President has suggested an 11-percent cut from $437 to $390, let me ask you to draw the line in the sand at $390 and not go any further beyond that. If it will help you, I will ask you to put some back on the theory that you can always go down but you can never come up.

Mrs. LLOYD. That might be a good thing for you to do.

Mr. STARK. If Tennessee could use another $10 million and we could use a couple of extra million in Livermore, I suppose that you and I could strike an agreement on that.

Second, I would like to talk about something a little bit more mundane that I think is very important to the future of our national laboratory system. That is the question of community participation and community responsibility. For those of us who enjoy the prestige and economic benefits of having important research centers located in our districts, but also to ask your committee to consider oversight into the effects that these centers have on our community. I'm going to suggest just a couple of areas in which I have found the people in the city of Livermore to be less than satisfied with the way the lab has treated the local community.

I think that with these facilities we have to remember that it is the communities that provide the residential setting for the many scientists and the schools and the shopping and the life that makes it easy for us to attract scientists. In many cases, these communities are not in such desireable places as Tennessee and California and we have to have other things to offer the employees.

We've had some problems of ground water contamination in our area that can reasonably be traced to the Lawrence Lab. This Congress last year provided some $17 million, I think, to buy up land around the Lawrence Lab as a kind of a moat, on the theory that somehow terrorists could come and destroy it if we don't keep 300 or 400 yards away. While I questioned the use of that $17 million, it was awarded to the lab. What I resented was that the Lab ran out and tried to buy it before the local communities could include it in an improvement district where they would have paid their fair share of road paving and sewer hookup. It's kind of a nickel and diming way to treat a community that really doesn't get any tax revenue from these additional lands being taken out of the public domain.

We've been fighting about a stoplight. Children have been endangered because of the traffic from the commuters. While I recognize that there is no present legal way for us to provide $80,000 for a stoplight, I've always felt that a little cooperation might go a long way toward making the relations easier for this Member of Congress, and I'm sure the same thing goes on in every community.

Mrs. LLOYD. Do you think there should be some oversight hearings on this?

Mr. STARK. I think it would be interesting to see how we are behaving in the country in terms of being good neighbors. Are the Federal programs aiding to the quality of life or detracting? I think that's something that your committee certainly should take on and I would certainly encourage you to do that and I would like to help insofar as California.

Mrs. LLOYD. This committee will take it under consideration.

Mr. STARK. I certainly appreciate it. I thank you for your past considerations and your valiant efforts to see this question of fusion continue. I would like to help, if I can, in the future. If not to vote for it on the floor, let's see if we can twist an arm or two with the rest of my California colleagues. I thank the committee very much for having me here today.

[The prepared statement of Mr. Stark follows:]

FORTNEY H. (PETE) STARK
9TH DISTRICT, CALIFORNIA

COMMITTEES
WAYS AND MEANS
DISTRICT OF COLUMBIA
SELECT NARCOTICS

CONGRESS OF THE UNITED STATES
HOUSE OF REPRESENTATIVES
WASHINGTON, D.C. 20515

STATEMENT OF CONGRESSMAN FORTNEY H. (PETE) STARK
BEFORE THE SUBCOMMITTE ON ENERGY RESEARCH AND PRODUCTION
COMMITTEE ON SCIENCE AND TECHNOLOGY

MARCH 18, 1985

ON THE FY86 FUSION ENERGY BUDGET AND THE LAWRENCE
LIVERMORE NATIONAL LABORATORY

STATEMENT OF CONGRESSMAN FORTNEY H. (PETE) STARK BEFORE THE SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION, SCIENCE AND TECHNOLOGY COMMITTEE

MARCH 18, 1985

ON FUSION ENERGY RESEARCH AND THE LAWRENCE LIVERMORE NATIONAL LABORATORY

Madame Chair, Members of the Committee:

I appreciate the opportunity to appear today.

I am here to ask two things:

First, please do not make any deeper cuts in the civilian nuclear fusion research program than are already proposed in the President's budget, and

Second, as you oversee the work of the National Laboratories this year, I hope you can inspire them to be better citizens in their communities.

Last year, in the appropriations process, the Congress made some cuts in the fusion research budget. This year, the President is asking for an 11% cut, from $437 to $390 million. I am told by the staff at Lawrence Livermore National Lab that this will mean the lay-off of about 100 engineers from the fusion energy project. Princeton, I am told, will possibly have to do with about 180 less staff. Clearly, this will slow up our progress and postpone the day of energy breakeven; clearly this will give the petroleum age a few extra years to hold the world hostage to dwindling supplies and soaring prices. But I am told this budget will not derail or destroy the Nation's fusion program and the gains that have been made--only slow the progress.

But if there are further cuts, if Congress makes deeper cuts, as it did last year, then the results will be disastrous. We will start to lose the key players with a life-time work on the potentials of fusion energy. Younger, bright physicists will not be attracted to the field, and the national policy of having several options to the fusion future of the world--mirrors and tokamaks--will probably die. We will surrender the field of fusion energy and the cutting edge of advanced physics to the Europeans, Soviets, and Japanese.

I know that in my Committee, all we ever get is a parade of witnesses asking for more of something. I would like to shock you a bit by just asking you not to cut more than the President asks!

I enjoy supporting the fusion program. It has made great progress and advanced the frontiers of science. It holds out the possibility of limitless, pollution free energy for the 21st Century. In my own Congressional District, it is the part of the work of the Lawrence Livermore National Lab that has, really, complete community support.

As you know, the Lab's work--particularly on weapons and now the Star Wars system--is highly controversial. Many of us are schizophrenic about the Lab. We admire the brilliance and competence of the researchers and yet we wish that this type of work did not need to be done in a world already loaded with weapons.

But I would also ask your help during the coming year in overseeing--perhaps through local field hearings--how the National Labs, particularly the Lawrence Livermore Lab, deal with the communities were they are located. Support for the Lab in the

Livermore community is made unnecessarily schizophrenic because--frankly--they are often a bad neighbor. In the last year, we have had problems ranging from the very serious--such as groundwater contamination from the Lab which they have dragged their feet in dealing with--to serious--such as refusing to find ways to help the local community in meeting extra security costs due to the Lab and helping with a traffic danger caused by Lab motorists at a local school crossing. Congress gave the Lab extra money to buy buffer land around the Lab for security purposes, yet they alienated the local community and landowners in the way they proceeded. Given the way they handled the buffer land issue, I would much rather see the $10 million they are spending on new land used to keep the fusion research program on track!

In short, I think the national fusion energy research program deserves an A+ and the full support of the Congress. I also think this Committee could help the cause of science support in this country if you made sure the local Labs are better neighbors.

Thank you for your help on both these issues.

Mrs. LLOYD. We thank you for your input and for your support for the fusion program. We hope that you can inspire more of your colleagues to give a little bit more aid to our program.

I don't believe there are any questions. We thank you for being here.

Our first panel from panel one is Dr. Ronald Parker of MIT. Dr. Parker, your entire written statement will be included in the hearing record; you may summarize or proceed as you wish. We welcome you to the hearing today.

I think the entire panel should come up at this time; Len Reichle, Dr. Ohkawa, and Dr. Dean.

STATEMENT OF DR. RONALD R. PARKER, ASSOCIATE DIRECTOR, MIT PLASMA FUSION CENTER AND HEAD, ALCATOR GROUP

Dr. PARKER. Madam Chairman, I am grateful for the opportunity to address this subcommittee and present my views on the subject of a low-cost ignition device for the Magnetic Fusion Energy Program.

I believe that achieving and studying the ignited plasms state is an absolutely essential and correct next step for the Magnetic Fusion Energy Program. I also believe that expecting to accomplish this goal in a tokamak of modest size—major radius not larger than 2 meters, high-field—8-10 Tesla—tight time scale—not more than 6 years construction time—and modest cost—less than $0.4 billion in an existing facility—is reasonable and that we should proceed in this direction as soon as possible. I will present arguments in support of this position in the remainder of my testimony.

In order to establish the significance of ignition, I will first take a few minutes to discuss the energy balance in fusion plasmas at various stages of fusion energy development. In the present stage of fusion research, plasmas are routinely produced in the tokamak configuration with parameters close to, but still significantly less than, those which will be required for producing net fusion power. Although remarkable progress has been made in recent years, the power supplied to maintain the plasma state in every one of our most advanced experiments is still much greater than the fusion power which would be produced even using D-T fuel. The situation is illustrated schematically in figure 1A.

As plasma confinement improves, and as more heat is added, the fusion power produced relative to the power lost will grow, and a state referred to as energy break-even will occur. As illustrated by figure 1B, the fusion power produced will then equal the power supplied by the external sources for example, by neutral beams and/or radio-frequency systems. Achieving this condition, at least in an approximate sense, has been, and continues to be the goal of TFTR. Incidentally, the last year's research on TFTR has been very encouraging in this regard, and current expectations are that near break-even conditions will be produced by the end of 1986. This would obviously be a very significant landmark in the history of fusion energy research and development.

In a D-T plasma operating close to reactor conditions, a fraction of the net fusion power produced is predicted to be self-absorbed by the plasma and thereby contribute to the overall power balance. In today's experiments this power is negligible; in the break-even experiment, it is about 20 percent of the total power required. As plasma confinement continues to improve however, more fusion power will be produced relative to the power loss, and a stage occurs where the self-absorbed power is equal to the power lost by the plasma. In the case of the D–T reaction, helium nuclei, also known as alpha particles, produce that power which can be self-absorbed. This is the state referred to as ignition, and the most significant feature of the ignited plasma is that no power is required to be supplied by the external sources; the plasma is then self-sustaining and produces power without diversion of any power back to the plasma to make up for the inherent power losses.

Because of fundamental energy conversion inefficiencies as well as the additional cost penalties imposed by high recirculating power, the external power used to heat the plasma in an economic electricity-producing fusion reactor must be kept small in relation to the neutron power release. It follows that the major source of plasma heat must be the energetic charged particles produced by the fusion reaction itself. This means that all fusion reactors, regardless of specific embodiment must operate at or close to an ignited state. We therefore see that an ignited state is an intrinsic feature of an efficient fusion reactor. Since ignition is considered to be a sine qua non of fusion energy, it follows that the particular magnetic confinement configuration initially used to attain and study the scientific aspects of an ignited plasma is of secondary importance.

Taking into account the status of all magnetic confinement concepts, one finds that the tokamak is the only configuration presently capable of achieving ignition with a reasonable probability of success. This is graphically demonstrated in figure 2, which shows the results of various toroidal experiments relative to the plasma conditions of temperature and confinement quality necessary for achieving ignition. The ignition region is shown shaded in the upper-right hand corner of this diagram. The figure shows the consistent and remarkable progress, mainly made in the United States thanks in part to the continued support of this Committee, in approaching ignition conditions using the tokamak configuration. Indeed, the minimum temperature for ignition has now been achieved in a number of devices, specifically PLT, PDX, and most recently TFTR at Princeton Plasma Physics Laboratory, and D-III at General Atomic Technologies. While most of these experiments were heated with neutral beams, during the past year RF-heated plasmas in PLT were produced with temperatures in excess of the ignition threshold. This has special significance in the context of a compact ignition experiment which I will discuss later. The minimum insulation parameter, or N–TAU required for break-even has been achieved in Alcator C at MIT, and this record value is only about a factor of two below the minimum necessary for ignition. One can conclude from these data that the stage is set for achieving ignition in a tokamak. In even stronger terms, the achievements represented by the data points in figure 2 were made only with tireless and often heroic contributions from many researchers dedicated to the development of fusion energy. In my opinion, history will not judge us kindly if we do not take full advantage of the opportunity presented to us now to cross the ignition frontier.

The question of appropriate scale of an ignition device has been hotly debated within the U.S. fusion community during the last few years. It now appears prudent, and in fact essential in view of the present budget outlook, to construct the minimum cost device which can still address the most intrinsically important scientific issues associated with the ignited plasma state. While a very technical set of questions and issues can be formulated relating to the unknowns in an ignited plasma, the key unknown can be summarized by the following general question: what is the efficiency and confinement law governing the flow of heat and particles in a self-heated plasma? It will be necessary to answer this question whatever course the future development of magnetic fusion energy may take. It is worth noting that, as in the case of netural beam and RF methods of plasma heating, much of the research and development associated with undertaking the study of a-particle heating in a tokamak will be directly transferable to the development of other fusion energy concepts.

Having addressed the question of scale, at least in a philosophical sense, I will now turn to the specific path which I believe the ignition experiment should take. The route which should be followed can be discovered by re-examination of figure 2. The device with highest N-TAU is Alcator C, in which an N-TAU value of nearly 10 14 cm-3 sec was produced in 1983. This device was also the least expensive of those shown in the figure to construct, having cost approximately $6.5 million when it was built in the mid-1970's. The reason that the performance/cost ratio is so high is that Alcator relies on the use of very high magnetic fields in a relatively small volume torus. Since the cost of tokamak devices increases rapidly with size but slowly with magnetic fields, the cost of a compact, high-field tokamak is much less than that of a large, low-field device. Fortunately, as the laws of tokamak physics have shown, the enhancement in performance due to the high magnetic field more than offsets the gains which accrue due to large size.

One problem with the compact, high-field device in the past has been the difficulty in heating by the well established method of neutral beams. This is because neutral beam heating requires access to the plasma chamber which is difficult to attain in a compact design. However relatively recent advances in RF heating experiments, both in Alcator C and in the PLT device have given us confidence that a compact device can be heated to ignition temperatures by use of RF heating near the ion cyclotron frequency. I conclude from this that the probability of achieving ignition in a relatively compact tokamak using RF heating is high, and that this is an appropriate and highly desirable step for the Magnetic Fusion Energy Program to take at this time. The cost should be in the range of $400 million in an existing facility.

In summary, I have tried to convey in my testimony the following major points: One, achieving the ignited plasma state is an intrinsic step in the development of fusion energy, regardless of the ultimate reactor embodiment; two, the tokamak concept is the only one capable of ignition at this time and for the foreseeable future; three, of the various tokamak options, a relatively compact, high-field device heated with radio-frequency power is the most cost-effective approach; and four, the Magnetic Fusion Energy Program should plan an ignition experiment based on this approach, making maximal use of existing resources.

Thank you, Madam Chairman.

[The prepared statement of Dr. Parker follows:]

THE IGNITION STEP IN THE DEVELOPMENT OF FUSION ENERGY

Testimony presented to

THE SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

COMMITTTEE ON SCIENCE AND TECHNOLOGY

U.S. HOUSE OF REPRESENTATIVES

WASHINGTON, D.C.

by

Ronald R. Parker
Associate Director, MIT Plasma Fusion Center
and
Head, Alcator Group

March 18, 1985

Madam Chairman, I am grateful for the opportunity to address this Subcommittee and present my views on the subject of a low-cost ignition device for the Magnetic Fusion Energy Program.

I believe that achieving and studying the ignited plasma state is an absolutely essential and correct next step for the Magnetic Fusion Energy Program. I also believe that expecting to accomplish this goal in a tokamak of modest size (major radius not larger than 2 meters), high-field (8-10 Tesla), tight time scale (not more than six years construction time) and modest cost (less than $0.4B in an existing facility) is reasonable and that we should proceed in this direction as soon as possible. I will present arguments in support of this position in the remainder of my testimony.

Ignition as an Intrinsic Step in Fusion Energy Development

In order to establish the significance of ignition, I will first take a few minutes to discuss the energy balance in fusion plasmas at various stages of fusion energy development. In the present stage of fusion research, plasmas are routinely produced in the tokamak configuration with parameters close to, but still significantly less than, those which will be required for producing net fusion power. Although remarkable progress has been made in recent years, the power supplied to maintain the plasma state in every one of our most advanced experiments is still much greater than the fusion power which would be produced even using D-T fuel. (Present experiments use Hydrogen or Deuterium fuel for convenience). The situation is illustrated schematically in Figure 1A.

As plasma confinement improves, and as more heat is added, the fusion power produced relative to the power lost will grow, and a state referred to as energy breakeven will occur. As illustrated by Figure 1B, the fusion power produced will then equal the power supplied by the external sources e.g., by neutral beams and/or radio-frequency systems. Achieving this condition, at least in an approximate sense, has been, and continues to be the goal of TFTR. Incidentally, the last year's research on TFTR has been very encouraging in this regard, and current expectations are that near breakeven conditions will be produced by the end of 1986. This would obviously be a very significant landmark in the history of fusion energy research and development.

In a D-T plasma operating close to reactor conditions, a fraction of the net fusion power produced is predicted to be self-absorbed by the plasma and thereby contribute to the overall power balance. In today's experiments this power is negligible; in the breakeven experiment, it is about 20% of the total power required. As plasma confinement continues to improve however, more fusion power will be produced relative to the power loss, and a stage occurs where the self-absorbed power is equal to the power lost by the plasma (Figure 1C). In the case of the D-T reaction, helium nuclei, also known as alpha particles, produce that power which can be self-absorbed. This is the state referred to as ignition, and the most significant feature of the ignited plasma is that no power is required to be supplied by the external sources; the plasma is then self-sustaining and produces power without diversion of any power back to the plasma to make up for the inherent power losses.

Because of fundamental energy conversion inefficiencies as well as the additional cost penalties imposed by high recirculating power, the external power used to heat the plasma in an economic electricity-producing fusion reactor must be kept small in relation to the neutron power release. It follows that the major source of plasma heat must be the energetic charged particles produced by the fusion reaction itself. This means that all fusion reactors, regardless of specific embodiment must operate at or close to an ignited state. We therefore see that an ignited state is an intrinsic feature of an efficient fusion reactor. Since ignition is considered to be a sine qua non of fusion energy, it follows that the particular magnetic confinement configuration initially used to attain and study the scientific aspects of an ignited plasma is of secondary importance.

Achieving Ignition in a Tokamak

Taking into account the status of all magnetic confinement concepts, one finds that the tokamak is the only configuration presently capable of achieving ignition with a reasonable probability of success. This is graphically demonstrated in Figure 2, which shows the results of various toroidal experiments relative to the plasma conditions of temperature (vertical axis) and confinement quality (or "N-TAU" - horizontal axis) necessary for achieving ignition. The ignition region is shown shaded in the upper-right hand corner of this diagram. The figure shows the consistent and remarkable progress, mainly made in the US thanks in part to the continued support of this Committee, in approaching ignition conditions using the tokamak configuration. Indeed, the minimum temperature for ignition has now been achieved in a number of devices, specifically PLT, PDX, and most recently TFTR at Princeton Plasma Physics Laboratory, and D-III at General Atomic Technologies. While most of these experiments were heated with neutral beams, during the past year RF-heated plasmas in

PLT were produced with temperatures in excess of the ignition threshold. This has special significance in the context of a compact ignition experiment which I will discuss later. The minimum insulation parameter, or "N-TAU" required for breakeven has been achieved in Alcator C at MIT, and this record value is only about a factor of two below the minimum necessary for ignition. One can conclude from these data that the stage is set for achieving ignition in a tokamak. In even stronger terms, the achievements represented by the data points in Figure 2 were made only with tireless and often heroic contributions from many researchers dedicated to the development of fusion energy. In my opinion, history will not judge us kindly if we do not take full advantage of the opportunity presented to us now to cross the "ignition frontier"!

The question of appropriate scale of an ignition device has been hotly debated within the US fusion community during the last few years. It now appears prudent, and in fact essential in view of the present budget outlook, to construct the minimum cost device which can still address the most intrinsically important scientific issues associated with the ignited plasma state. While a very technical set of questions and issues can be formulated relating to the unknowns in an ignited plasma, the key unknown can be summarized by the following general question:

> What is the efficiency and confinement law governing
> the flow of heat and particles in a self-heated
> plasma?

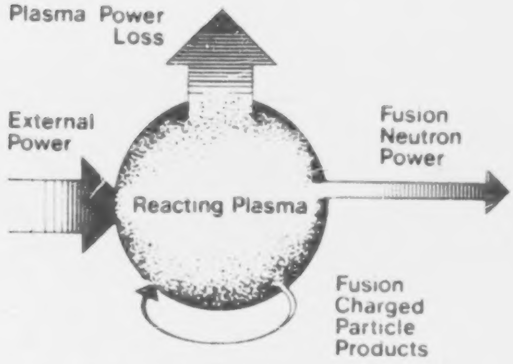
It will be necessary to answer this question whatever course the future development of magnetic fusion energy may take. (Admittedly, some details of the answer may depend ultimately on the particular magnetic confinement configuration). It is worth noting that, as in the case of neutral beam and RF methods of plasma heating, much of the research and development associated with undertaking the study of α-particle heating in a tokamak will be directly transferable to the development of other fusion energy concepts.

A Cost-Effective Path to Tokamak Ignition

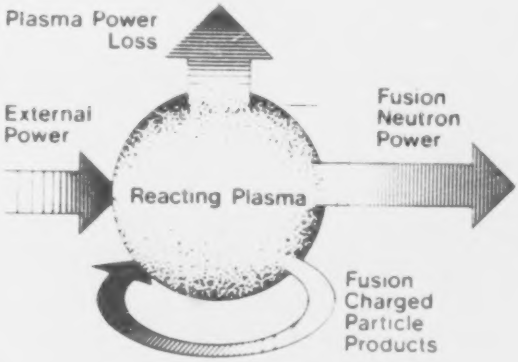
Having addressed the question of scale, at least in a philosophical sense, I will now turn to the specific path which I believe the ignition experiment should take. The route which should be followed can be discovered by re-examination of Figure 2. The device with highest N-TAU is Alcator C, in which an $n(0)\tau_E$ value of nearly 10^{14} cm^{-3} sec was produced in 1983. This device was also the least/expensive of those shown in the Figure to construct, having cost approximately $6.5M (including prime power) when it was built in the mid 1970's. The reason that the performance/cost ratio is so high is that Alcator relies on the use of very high magnetic fields in a relatively small volume torus. Since the cost of tokamak devices increases rapidly with size but slowly with magnetic fields, the cost of a compact, high-field tokamak is much less than that of a large, low-field device. Fortunately, as the laws of tokamak physics have shown, the enhancement in performance due to the high magnetic field more than offsets the gains which accrue due to large size.

One problem with the compact, high-field device in the past has been the difficulty in heating by the well-established method of neutral-beams. This is because neutral beam heating requires access to the plasma chamber which is difficult to attain in a compact design. However relatively recent advances in RF heating experiments, both in Alcator C (400 eV temperature rise with 400kW in 1984) and in the PLT device (5 keV temperature rise with 5MW in 1985) have given us confidence that a compact device can be heated to ignition temperatures by use of RF heating near the ion cyclotron frequency. I conclude from this (and also from consideration of the confinement laws governing tokamaks) that the probability of achieving ignition in a relatively compact (major radius less than 2 meters), high-field (field strength 8-10 Tesla) tokamak using RF heating is high, and that this is an appropriate and highly desirable step for the magnetic fusion energy program to take at this time. The cost should be in the range of $400M in an existing facility.

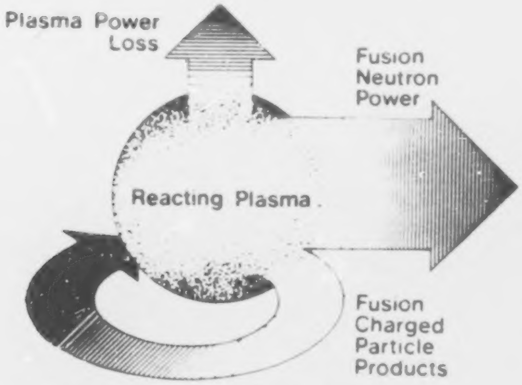
In summary, I have tried to convey in my testimony the following major points: 1) achieving the ignited plasma state is an intrinsic step in the development of fusion energy, regardless of the ultimate reactor embodiment; 2) the tokamak concept is the only one capable of ignition at this time and for the foreseeable future; 3) of the various tokamak options, a relatively compact, high-field device heated with radio-frequency power is the most cost-effective approach; and 4) the magnetic fusion energy program should plan an ignition experiment based on this approach, making maximal use of existing resources.


Plasma Power Loss
External Power
Fusion Neutron Power
Reacting Plasma
Fusion Charged Particle Products

a. PRESENT-DAY FUSION EXPERIMENTS


Plasma Power Loss
External Power
Fusion Neutron Power
Reacting Plasma
Fusion Charged Particle Products

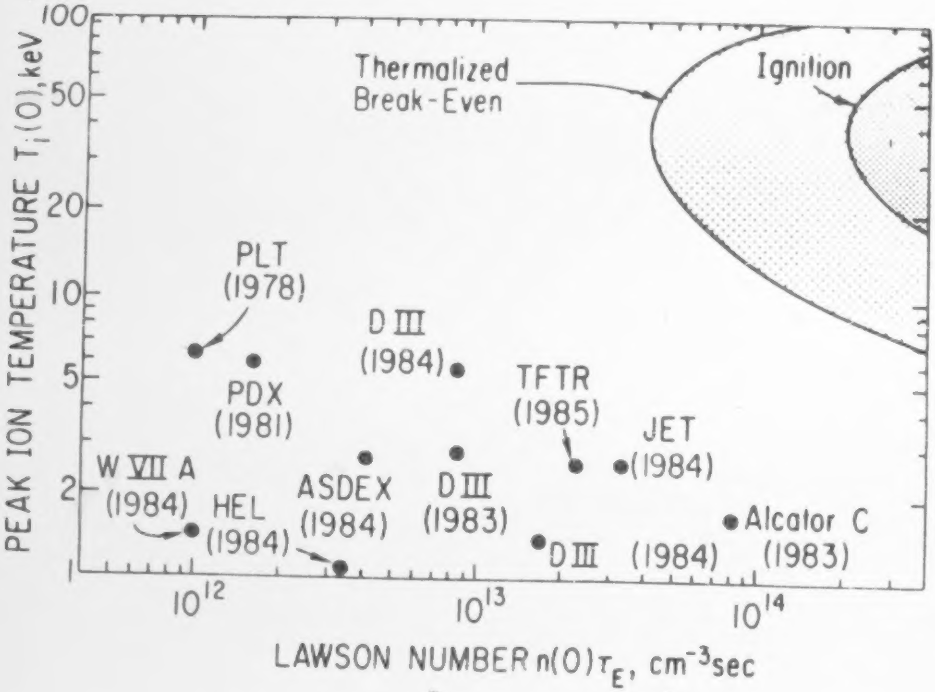
b. A BREAKEVEN FUSION REACTOR


Plasma Power Loss
Fusion Neutron Power
Reacting Plasma
Fusion Charged Particle Products

c. AN IGNITED FUSION REACTOR

SCHEMATIC POWER BALANCE IN
VARIOUS STEPS OF FUSION ENERGY DEVELOPMENT

FIGURE 1



RECENT PROGRESS OF TOROIDAL EXPERIMENTS TOWARDS THE REACTOR CONFINEMENT REGIME

FIGURE 2

Mrs. LLOYD. Thank you very much, Dr. Parker. That was well presented.

Dr. Ohkawa, you may proceed.

STATEMENT OF DR. TIHIRO OHKAWA, VICE PRESIDENT, GA TECHNOLOGIES, INC.

Dr. OHKAWA. Madam Chairman, members of the committee, I am Dr. Tihiro Ohkawa, vice president of GA Technologies. Thank you for your support of the National Fusion Program and GA's part in that effort.

The present technological leadership of the United States is due in large measure to the breadth and depth of our Nation's scientific base. The Magnetic Fusion Energy Program does good science and contributes in a significant way to honing high-technology skills in a variety of disciplines. Although recent adverse budgetary developments will of necessity delay the schedule for developing a fusion energy option, there has been minimal compromise in the integrity of the science. Indeed, the program is now structured to ensure that a secure scientific base exists prior to making key program decisions. This is as it should be. Given the status of existing energy supplies, the need for a fusion energy option is perhaps two or three decades hence unless environmental or political events demand an earlier implementation. This permits a reasoned judgement on the confinement approach best suited for end-use applications. Such a reasoned judgement requires both strong theoretical underpinnings and a confirmation of the theory in plasmas that approximate end-use conditions.

The Magnetic Fusion Program plan prepared by the Department of Energy identifies this search for the optimum confinement systems as the key technical issue for magnetic fusion. For the next few years, the bulk of the program resources will be devoted to this undertaking. The Tokamak approach will be used as the spearhead to access new regimes while alternative confinement approaches will be used to pinpoint the dependence of the science on the details of the magnetic geometry.

Today the fundamental shortcoming of fusion science is the absence of data in a burning plasma, defined as a plasma that is sustained by the fusion fire. Because fundamentally new science may emerge from a burning plasma experiment, such a step is an inescapable program element. Unfortunately, the dynamics of such a process cannot be simulated in existing devices, in which the plasma is sustained by sources of power under the control of the experimentalist.

Thus a new machine is required. Only the Tokamak is sufficiently advanced to meet this challenge in the near term. My view is that we should begin immediately to design a burning plasma Tokamak. Furthermore, I feel that the highest priority of both the Tokamak experimental effort and the technology development effort should be to provide confirmation of the design decisions. The continuous string of advances in the Tokamak Program make me very confident that such a process would lead to a successful burning plasma experiment.

By isolating the burning plasma issue from a number of nuclear technology objectives, the cost of the experiment can be kept to a manageable level. The proper perspective to view the cost issue is expressed in the Magnetic Fusion Program plan in the phrase "to provide the broadest understanding at minimum cost."

Because the critical program need is the understanding of the physics of a burning plasma, it is not enough for the device to perform a demonstration. The device must also be capable of yielding an extensive quantity of high quality data.

Of particular concern is the ability to keep state-of-the-art hardware in good working condition when faced with the complexities of remote maintenance. I firmly believe that we should not cut corners in areas that govern the definitiveness with which burning plasma physics can be investigated. The price for the understanding to be gained is a wise investment in the future of fusion research. Even with this approach, however, the cost of the device itself will not dominate the cost of the experiment. Indeed, the largest cost items for the compact burning plasma experiment is the facilities, an area where a clever program strategy can bring large cost savings. A number of existing sites possess resources that could be used to offset a significant portion of the overall program costs.

Turning to broader program issues, I wish to state my support of the Magnetic Fusion Program plan. It offers a sensible strategy for a balanced, but somewhat slower-paced, program to harness thermonuclear power. The fiscal year 1986 magnetic fusion energy budget submitted is the minimum level required to keep the program logic intact. Continuing to make progress in the face of two consecutive cuts from the fiscal year 1984 budget level is reflected in two new elements of program strategy embodied in the Magnetic Fusion Program plan. The first is a reduced emphasis upon future large facilities which simultaneously serve major scientific and technological missions. This is exemplified by the present conception of the burning plasma experiment, where the technology is regarded as subservient to the scientific mission and the cost constraints.

The second new element of program strategy is increased reliance upon international cooperation. While I find great appeal in the potential benefits of international cooperation, our reliance upon it should not extend to abrogating technical responsibility in any of the research areas that are critical to the fusion energy option. We may choose to accomplish individual program steps via joint ventures but we must remain self-reliant in all the key scientific and technological disciplines. After all, who would want to cooperate with us if our capabilities were inferior to their own. The burning plasma experiment is a natural candidate for international cooperation. The United States must take the initiative in such a venture, however. The preferred approach is to commit to such a project and bring in partners to expand the program scope.

I wish to close by pointing out the remarkable parallelism between the existing program at GA Technologies and the program structure embodied in the Magnetic Fusion Program plan. Our largest program element is Doublet III, now in the midst of conversion to DIII–D, a large dee-shaped Tokamak. DIII-D will play a pivotal role in preparation for a burning plasma experiment. The search for the optimal magnetic geometry is the major GA program focus, both through DIII-D and through OHTE, a confinement scheme recently pioneered at GA that has already had a number of significant experimental successes. Although less seasoned than the Tokamak approach, OHTE shares with other high power density approaches the advantage of a shorter product cycle. Thus, advances in the science could trigger major program strides in a relatively brief period. It is to be noted that OHTE has been supported entirely by private funding from major corporations, and thus accomplishes directly the technology transfer objective espoused prominently in the Magnetic Fusion Program plan. Further evidence of the similarity between the GA program and the blueprint for the national effort is the central role played by international cooperation. Indeed, the cooperative agreement with the Japan Atomic Energy Research Institute for joint research on Doublet III is the largest cooperative fusion venture yet undertaken by the United States.

To reiterate the substance of my message: By following the strategy outlined in the Magnetic Fusion Program plan, we can conduct a high quality, productive research program at the budget level submitted. Further budget erosion would strike at the core of the program logic. We should begin now to plan for a burning plasma Tokamak experiment, building on existing program resources. Accomplishing such a project by international cooperation has appeal if the United States takes the leadership role.

Your continued strong support of the Magnetic Fusion program is greatly appreciated. Thank you.

[The prepared statement of Dr. Ohkawa follows:]

Testimony before the House Science and Technology Committee
Energy Research & Production Subcommittee
on the
FY 1986 Magnetic Fusion Energy Budget
by
Dr. Tihiro Ohkawa, Vice President
GA Technologies, Inc.
March 11, 1985

Madame Chairman, Members of the Committee, I am Dr. Tihiro Ohkawa, Vice President of GA Technologies, with responsibility for magnetic fusion research and development. Thank you for inviting me to testify before you today on the Fiscal Year 1986 Magnetic Fusion Energy Budget. More importantly, thank you for your support of the national fusion program and GA's part in that effort.

GA Technologies has accumulated the broadest base of experience in magnetic fusion of any industrial concern in the world. Paralleling the history of the field, GA's involvement in fusion research progressed from primitive benchtop experiments in the 1950's and 1960's to today's sophisticated devices that themselves are an important driver of advanced technology. Thirty years of dedication to the harnessing of fusion power has given me some insight into the proper direction for the magnetic fusion program. I am honored to have the opportunity to share my thoughts on this subject with you.

The present technological leadership of the United States is due in large measure to the breadth and depth of our nation's scientific base. The Magnetic Fusion Energy Program does good science and contributes in a significant way to honing high-technology skill in a variety of disciplines. Although recent adverse budgetary developments will of necessity delay the schedule for developing a fusion energy option, there has been no compromise in the integrity of the science. Indeed, the program is now structured to insure that a secure scientific base exists prior to making key program decisions. This is as it should be. Given the status of existing energy supplies, the need for a fusion energy option is perhaps two or three decades away unless environmental or political "events" demand an earlier implementation. This permits a reasoned judgement on the confinement approach best suited for end-use applications. Such a reasoned judgement requires both strong theoretical underpinnings and a confirmation of the theory in plasmas that approximate end-use conditions.

The Magnetic Fusion Program Plan prepared by the Department of Energy identifies this search for the optimum confinement systems as the key technical issue for magnetic fusion. For the next few years, the bulk of the program resources will be devoted to this undertaking. The tokamak approach will be used as the spearhead to access new regimes while alternative confinement approaches will be used to pinpoint the dependence of the science on the details of the magnetic geometry.

Today, the fundamental shortcoming of fusion science is the absence of data in a burning plasma, defined as a plasma that is sustained by the fusion fire. Because fundamentally new science may emerge from a burning plasma experiment, such a step is an inescapable program element. Unfortunately, the dynamics of such a process cannot be simulated in existing devices, in which the plasma is sustained by sources of power under the control of the experimentalist.

Thus a new machine is required. Only the tokamak is sufficiently advanced to meet this challenge in the near term. My view is that we should begin immediately to design a burning plasma tokamak. Furthermore, I feel that the highest priority of both the tokamak experimental effort and the technology development effort should be to provide confirmation of the design decisions. The continuous string of advances in the tokamak program make me very confident that such a process would lead to a successful burning plasma experiment.

By isolating the burning plasma issue from a number of nuclear technology objectives, the cost of the experiment can be kept to a manageable level. The proper perspective to view the cost issue is expressed in the Magnetic Fusion Program Plan in the phrase "to provide the broadest understanding at minimum cost", a theme that I wish to elaborate upon.

Because the critical program need is the understanding of the physics of a burning plasma, it is not enough for the device to perform a demonstration. The device must also be capable of yielding an extensive quantity of high quality data. Thus the design of the burning plasma experiment should incorporate the following feature, which would not be present in the demonstration alternative:

1. Access for a full complement of scientific instruments to measure and analyze plasma behavior.
2. Flexibility to explore a variety of operating conditions.
3. High availability, so that adequate operating experience can be obtained.

The last of these issues, that of availability, has the greatest cost impact. Of particular concern is the ability to keep state-of-the-art hardware in good working condition when faced with the complexities of remote maintenance. I firmly believe that we should not cut corners in areas that govern the definitiveness with which burning plasma physics can be investigated. The price for the understanding to be gained is a wise investment in the future of fusion research.

Even with this approach, however, the cost of the device itself will not dominate the cost of the experiment. Indeed, the largest cost item for the compact burning plasma experiment is the facilities, an area where a clever program strategy can bring large cost savings.

A number of existing sites possess resources that could be used to offset a significant portion of the overall program costs.

I must emphasize however, that a completely new torus is essential. In other words, while the U.S. and the international fusion programs can draw upon considerable existing resources to reduce the cost of a burning plasma experiment, none of the experimental devices themselves are suitable, even with substantial upgrading.

Turning to broader program issues, I wish to state my support of the Magnetic Fusion Program Plan. It offers a sensible strategy for a balanced, but somewhat slower-paced, program to harness thermonuclear power. The FY 1986 Magnetic Fusion Energy Budget submitted is the minimum level required to keep the program logic intact. Continuing to make progress in the face of two consecutive cuts from the FY 1984 budget level is reflected in two new elements of program strategy embodied in the Magnetic Fusion Program Plan. The first is a reduced emphasis upon future large facilities which simultaneously serve major scientific and technological missions. This is exemplified by the present conception of the burning plasma experiment, where the technology is regarded as subservient to the scientific mission and the cost constraints.

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To reiterate the substance of my message: by following the strategy outlined in the Magnetic Fusion Program Plan, we can conduct a high quality, productive research program at the budget level submitted. Further budget erosion would strike at the core of the program logic. We should begin now to plan for a burning plasma tokamak experiment, building on existing program resources. Accomplishing such a project by international cooperation has appeal if the United States takes the leadership role.

Your continued strong support of the magnetic fusion program is greatly appreciated. I will be happy to answer any questions you may have.

Mrs. LLOYD. We thank you, Dr. Ohkawa. Dr. Dean, please proceed with your testimony.

STATEMENT OF DR. STEPHEN O. DEAN, PRESIDENT, FUSION POWER ASSOCIATES

Dr. DEAN. Thank you, Madam Chairman. It's a pleasure to be back.

Mrs. LLOYD. It's a pleasure to have you back.

I would like to comment first in a broader context which is something that concerns me. Somehow the executive branch has abrogated responsibility for the development of energy technologies in this country on the grounds that it's the Federal responsibility simply to fund research and it's the private sector's job to do the technology development. I think the evidence is that they have effectively abrogated that responsibility and focused themselves on research. I think it's also true that the private sector is not picking up those energy technologies because they don't have profit potential in the next 5 or so years.

What has happened is there has developed a tremendous gap for things that pay off for the public benefit in the time scale of 20 years. These technologies are long lead-time, they take decades to develop, and nobody is paying for these technology developments right now. The plight that fusion finds itself in is in no way different from any of the other energy technologies where those technologies just aren't being paid for.

I think one thing is that fusion will probably not get back on track unless the Congress or the executive can find some way of designating the responsibility and the funding for these longer term technology developments that take decades to develop.

Since the Federal budget seems to be in so much trouble, it doesn't appear reasonable that one is going, in the near term, to get the Congress to fund these programs. It seems to me, therefore, that one ought to look for some off-budget ways of funding these types of projects. It seems to me that the industry that's going to benefit from these projects, which is the electrical utility industry and the oil industries, ought to be encouraged to fund these kinds of developments. That could either be by some kind of a user's tax that would go into an R&D fund, or some type of a tax incentive that would be strong enough to encourage industry to invest in things which pay off longer than 5 years down the road.

If we can't find some way of doing that, I'm not sure that we can get fusion or any of the energy technologies funded at the level that's commensurate with both the difficulty of the problem and the impact that they'll have in the next century. These energy technologies are worth trillions of dollars in the 21st century. That's the kind of industry we're trying to impact. The R&D for that industry ought to be commensurate with the fact that it's trying to impact a trillion dollar industry and not being run at a couple of billion dollars like the Energy Department is. This kind of R&D ought to be funded at the tens of billions of dollars a year. If it can't be in the Federal budget, it ought to come from some other source.

I'm not optimistic that we can get fusion rolling unless we can get all of the energy technologies with these long-lead payoffs funded at some reasonable level. Having said that, I must point out that fusion progress has been returning benefits commensurate with the funds that have been put into it. We've accomplished the things that we promised to do. We've built the machines we promised to build. We've got continuing progress on the research that we were funded to do. Therefore, I think the program stands before you with some pride at having accomplished what it was funded to do.

As you know, the last major experiments in this program were funded almost 10 years ago. If we don't commission some new ones and design future ones, we will be in a very difficult position to continue providing you the results 5 and 10 years from now. This is a serious problem.

There have been ideas coming along which are less expensive to test and I think that we can promise you that we can continue progress with less of an investment in the future than we had thought was going to be the case 5 years ago. There's an added risk in taking this approach because you will have to do some things that are a little bit smaller than the physicists might feel comfortable with. Everyone always likes a little margin for success and that costs money. If we can't afford that, maybe we can do it with a little higher risk and it will still work. I think we have a growing consensus in the program that this risk is reasonable and if you will fund us to do it, we can take it.

While progress in the program has continued, for the past few years there has been a continuing weakening of resolve in the executive to pursue these programs with a practical goal of producing something practical. There's been a tendency to more and more want to fund this work without promise of a practical product for the benefit of the science, in the hopes that something useful will come out. I think we need to continue to urge the executive branch to make near-term promises and objectives aimed at a practical product on this program.

We should not be funding this program with a goal that's 30 or 40 years in the future. We should expect something from this program in the 10- to 15-year period that's publicly understandable as progress toward a commercial product.

Within the constraints of the budget, I think the Department's program plan is reasonable but I do believe that they have slightly underfunded the systems and long-range planning and conceptual reactor design studies relative to the rest of the program. These kinds of studies which look longer-range at the commercial product and how to get there, are relatively cheap. But they are essential to assure the Congress and the taxpayer that the program intends to try to develop something that someone would want.

It concerns me greatly that many people in the public sector are worrying whether fusion is going to deliver something that anybody would buy. Since we can't build those things yet, the only way we can assure people that we're doing that is through these system studies. I believe that they are somewhat underfunded in the present circumstances.

Last, I would like to call your attention to the plight of the Inertial Confinement Fusion Program which has almost been put out of existence in the Department of Energy. They have buried it in the weapons budget. They have cut it by 50 percent. The program is a good program. It's made good progress. We should not allow the Department to make it an invisible part of the weapons program. It deserves to be its own program and it deserves to have its own identity.

In fact, that leads me to comment on something I think's going on in the Department of Energy which I think is obvious but needs to be said. That is, even though 4 years ago the executive said they were going to abolish the Department of Energy and they haven't done it yet, they have been systematically abolishing all of its energy programs by ramping them down every year while its weapons budget goes up every year. Pretty soon, there won't be any energy programs in the Department of Energy and fusion suffers as a part of that. All the energy programs suffer as a part of that. If we're going to have a Department of Energy, I think it ought to fund energy programs and we ought to pay attention to what's going on here that the executive is abolishing the Department without facing up to the problems it would face if it tried to do it directly.

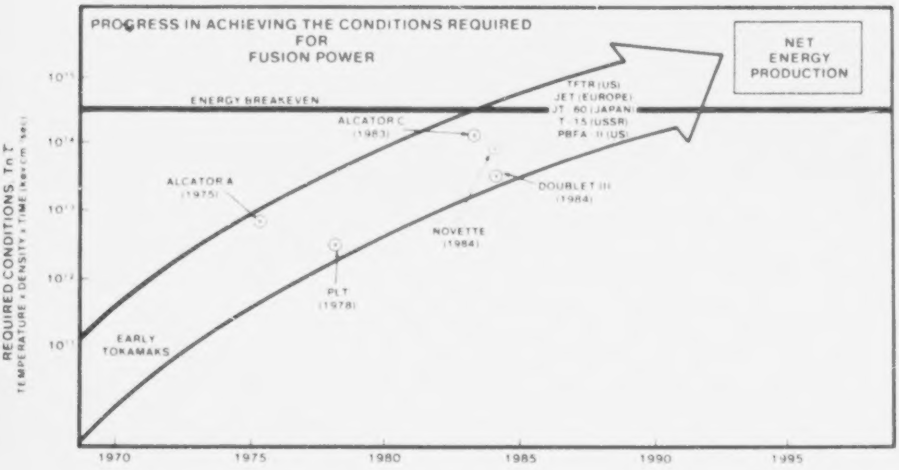
Thank you, very much.

[The prepared statement of Dr. Dean follows:]

FUSION POWER ASSOCIATES
2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879
(301) 258 0545

STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
MARCH 11, 1985

Progress in achieving the combined conditions required for producing fusion power (high temperature, high density and adequate time duration) is continuing at an impressive rate, as shown in the figure below. Also, just this past month, the Tokamak Fusion Test Reactor (TFTR) reached one of its design objectives: the production of plasma at 100 million degrees. This is an impressive accomplishment and gives us confidence that TFTR can reach its goal of reaching energy breakeven conditions.



"TO TAKE FROM THE SEA AND TO IMITATE THE SUN"

Unfortunately, while scientific progress has continued, there has been a weakening of government resolve to demonstrate the production of fusion power for practical purposes. Cuts in the program in FY 1985 and additional cuts proposed for FY 1986 would reduce program resources by nearly $100 million and have caused the Department of Energy to severely reduce the effort on our most ambitious projects: TFTR and MFTF-B. Many of us in the program believe that this will effectively transfer leadership of the world fusion effort to Europe and Japan. Opportunities for the participation of U.S. industry in fusion are significantly reduced.

In the face of policy restraints and budgetary reality, the DOE Office of Fusion Energy has devised a strategy that is reasonable under the circumstances. This strategy emphasizes pursuit in the near-term of several innovative ideas that are relatively inexpensive to test, strengthening of international collaboration and postponement of large hardware procurements. In describing the implementation of this strategy, however, I believe that the Office has underfunded efforts on systems analyses, long range planning and conceptual reactor design. A larger effort is needed to guide the research program and to assure the Congress and taxpayers that the fusion program is aimed at developing an attractive practical product.

While the responsibility of this Subcommittee is for magnetic fusion, I would urge you not to ignore the plight of the inertial confinement fusion program, which is included under DOE's weapons budget. The DOE has proposed to cut that program in half and to make it an "invisible" part of the weapons budget. I would consider it a national tragedy if DOE were allowed to decimate the inertial fusion program in this way.

The Board of Directors of Fusion Power Associates has approved a Policy Statement on fusion energy development which is attached.

FUSION POWER ASSOCIATES

BOARD OF DIRECTORS

POLICY STATEMENT

1985

The Board of Directors of Fusion Power Associates, on behalf of the association, urges that fusion power development be based upon the following:

RECOMMENDED POLICIES

- Research and development toward an economically-competitive fusion power system should receive high national and international priority.
- Engineering sciences, technology development, systems analyses and plasma sciences should all be considered essential elements in a balanced fusion effort.
- Timely commitments to necessary new and improved experimental facilities are needed to ensure continued program momentum and progress into the 1990's.

The above recommended policies are based upon the following:

FINDINGS AND RECOMMENDATIONS

- Fusion research, world-wide, continues to make outstanding progress.
- Concept improvements are emerging at a rapid rate, giving increasing confidence that an economically-competitive fusion power plant can be developed. Innovative ideas that reduce costs or accelerate knowledge should be expeditiously pursued in all aspects of the fusion program.
- It would be premature, at this stage, to judge which of the variety of magnetic and inertial fusion concepts will ultimately succeed commercially. This fact should not discourage use of the best available concepts in the design and construction of needed fusion test facilities.
- The fusion programs in the U.S., U.S.S.R., Europe and Japan have comparable accomplishments, facilities and momentum. The present rate of progress in the world program is based on capital investment commitments made in the 1970's. Progress will predictably diminish unless next step facilities are developed in a timely manner.
- International cooperative agreements have been a substantial factor in fusion progress and should be encouraged. Such agreements, however, are not an effective substitute for focused national efforts, needed national experimental facilities and subsequent engineering test devices.
- Governments should encourage and promote the full participation of industry in the planning, research and development as well as in engineering and operating aspects of fusion programs. If industry participates in the government - funded R & D programs now, industry will be better prepared to assess the commercial potential of fusion power in the future.
- Governments should foster innovation and optimize utilization of resources by encouraging and strengthening interrelationships among industry, laboratories and universities.
- Fusion - related programs in universities should be selectively strengthened and encouraged to ensure an adequate supply of engineers and physicists with knowledge in this field.
- The governments of the world have made a sustained investment in fusion research. The time has come to begin to capitalize on this investment by placing increased emphasis during the next decade on the engineering and systems design aspects of practical fusion energy systems.

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For further information, contact:
Fusion Power Associates
2 Professional Drive, Suite 248
Gaithersburg, Maryland 20879
(301) 258-0545

Mrs. LLOYD. Thank you. I certainly appreciate your statement, Dr. Dean. For the record, Dr. Dean is head of the Fusion Power Associates.

Next on our panel is Len Reichle, head of our Magnetic Fusion Advisory Panel. Please proceed, Len.

STATEMENT OF LEONARD F.D. REICHLE, MAGNETIC FUSION ADVISORY PANEL

Mr. REICHLE. I am Len Reichle, group vice-president and member of the board of directors of Ebasco Services Inc., and chairman of the Subcommittee's Magnetic Fusion Advisory Panel.

In general, I believe the state of the nation's Magnetic Fusion Energy Program is healthy, but not as healthy as it could be and should be. Ten years ago, the United States looked upon fusion as our most attractive alternate energy option. At that time, we said to ourselves essentially what the DOE 1986 Congressional Submission Program Overview on Magnetic Fusion Energy says:

> Fusion energy could have several attractive features including intrinsic safety and minimum environmental problems. Fusion would utilize a vast fuel reserve that is inexpensive, readily available, and free from threat of embargo.

Ten years ago, despite our limited knowledge of the science and technology of fusion, we recognized fusion's great potential and we resolved to get on with the show. We planned a Tokamak Fusion Test Reactor to demonstrate break-even in the early 1980's. We planned an engineering device to become operational in about 1990. We planned a Demonstration Fusion Power Reactor for the year 2000. With DOE funding and program support, the Princeton Plasma Physics Laboratory engaged Ebasco, who, along with Grumman Aerospace, our team subcontractor, designed and built the TFTR. The TFTR produced its first hydrogen plasma in 1982. The United States lead the world in the development of fusion energy.

Since then, the Nation's Fusion Energy Program has departed from an action-oriented program. It has departed from a program with discernible flagship projects aimed at the goal of demonstrating fusion-generated electricity in the year 2000. It has become what DOE calls "* * * the Nation's research effort to develop a major new energy source for this country in the next century." The program is less of an engineering-oriented program and more of a science-oriented program of the universities and the national laboratories.

Demonstration of break-even in TFTR has been delayed 2 years, but is now scheduled for 1986. On September 23, 1983, the magnetic Fusion Advisory Committee recommended that a Tokamak Fusion Core Experiment [TFCX] be initiated. On February 3, 1984, the Energy Research Advisory Board [ERAB] recommended that DOE commit to building what it called a burning core experiment—basically the generic model of TFCX. Our national laboratories, universities, and the fusion industry were in agreement to proceed with such a project. On February 14, 1984, PPPL issued a request for proposal to assist PPPL in the design of a TFCX. Several strong industrial teams spent hundreds of thousands of dollars on TFCX proposals. However, despite the strong support by the fusion community, on September 5, 1984, the RFP was withdrawn. Recently, on February 26, 1985, PPPL held an information meeting for a proposed ignition studies project. I hope such a next-step Tokamak project will be supported promptly by DOE and the Congress regardless of the funding level of the overall fusion budget.

As a Nation, we have lost leadership already in steelmaking, shipbuilding, radios and TV. In fact, in every major electronic market including computer equipment, communications equipment, office machines, and semiconductors and components, our imports are rising and our exports are falling. By failing sooner to have initiated the engineering design of a next-step magnetic confinement fusion device, we may already have assured our loss of leadership in fusion energy by 1990 or before. Let's do what we can to retain or regain leadership, even in a time of budget restraint.

What can we do? Madam Chairman, to suggest an answer to this question I shall address the three topics in your letter.

TOPIC NO. 1—THE REQUIREMENTS FOR MAINTAINING A STRONG, EFFECTIVE FUSION PROGRAM

Probably the most important requirement is the fostering of a perception by the U.S. Government and the public that it is important to develop fusion energy promptly because it is potentially our best alternate energy option. It has the potential of producing commercial electric energy from inexhaustible fuel resources in a cost-competitive manner, safer and more environmentally compatible than coal, nuclear fission, and the fast breeder reactor.

So our Advisory Panel, in its letter dated December 4, 1984 stated with respect to DOE's Magnetic Fusion Program Plan:

> We appreciate the budgetary realism of the plan, but we are distressed by the rate of progress envisioned for the near term.

Congressional appropriations for the Fusion Program were approximately $468 million in fiscal year 1984 and $437 million in fiscal year 1985. The administration's request for fiscal year 1986 is $390 million. This is a declining budget. But, a declining budget is no excuse for failing to proceed within the budget on a most-likely-to-succeed main-line effort, with only backup support for alternate options. Typically, in an industrial R&D program, the leading contender among development options is fully funded, and other options and technology development are relegated to lesser levels of support within the limits of available R&D funds. Among fusion options, the Tokamak by consensus of most of the fusion community has a sound theoretical base, has worldwide experience behind it, and is the "leading contender." Only last week, Dr. Harold Further announced at the ANS meeting in San Francisco that, with only 5 MW of neutral beams, the TFTR had achieved temperatures as high as previously achieved, e.g. 70 to 80 million degrees. Unofficially, I understand that it may have achieved more than 100 million degrees.

Let us build on the progress we are making. Let us promptly take the next step. Let us begin a burning-plasma experiment. Our advisory panel, in its letter on December 4, stated:

> We believe that, even within current budgetary restraints, it should be possible to undertake an affordable burning-plasma experiment by modifying existing facilities with that objective.

Also, our advisory panel, recognizing the need for balance in the program, said:

> We are pleased that the plan recognizes the need to address materials and nuclear technology issues, as well as the identification of an optimum confinement system.

TOPIC NO. 2—THE POTENTIAL FOR BUILDING A LOW-COST IGNITION DEVICE

Our advisory panel, on December 4, stated:

> A conceptual design for such a project should be prepared as soon as possible, aimed at a capital cost below $500 million and a project starting date in fiscal year 1987-88. The start of construction should be contingent on the demonstration, during 1986, of high-temperature plasma confinement results in TFTR that prove the feasibility of achieving ignition in such a device.

PPPL, on February 26, 1985, at its information meeting on the ignition studies project suggested that an ignition device could be built for $300-400 million if facilities and equipment at the TFTR complex are utilized. Ebasco has not made an independent estimate, but believes that, if project management, engineering design, procurement, installation, and assembly of the device is assigned to an acknowledgeable industrial subcontractor or industrial team, the probability is high that such a device can be built for less than $400 million.

Certainly, if the will to undertake such a project exists in the DOE, even a budget limited to $390 million per year could fund the project. Of course, some other programs would have to be curtailed. That would be a small price to pay for retaining or regaining world leadership in fusion energy.

TOPIC NO. 3—CONTINUING ROLE OF INDUSTRY IN THE FUSION PROGRAM

In December, our advisory panel stated:

> We believe that meaningful involvement of industry is essential to the healthy evolution of the United States fusion program and should be made explicit in the plan.

I regret to report that industrial involvement in the nation's fusion energy program has become less rather than more meaningful. Industrial subcontract work is being reduced as DOE-funded budgets at national laboratories and universities are reduced. But, what is of even greater importance is how our industry is involved. For conceptual design of the TFCX, PPPL proposed to mix and match various pieces of industrial proposals regarding the TFCX, and to manage and direct the engineering design themselves rather than to engage one of the industrial teams to perform the design, procurement and installation as a responsible industrial subcontractor. If the United States is to compete with Hitachi in Japan, we would be well advised to have a U.S. industrial company or industrial team in the responsible lead-role. That is how the TFTR was designed and built.

Last December, our advisory panel stated:

We support the strategy of seeking international collaboration on major fusion projects and believe that the initiation of the proposed burning plasma experiment will help to make the United States a strong partner.

Such international collaboration can be successful in the early programmatic stages of scientific research and development. However, as the program proceeds to an engineering state, with proprietary interests in systems engineering and hardware, intergovernment collaboration inevitably becomes less effective.

Let us not permit international collaboration to be a substitute for strong U.S. Government support of our national fusion energy program. At a time when the U.S. National Science Foundation is spending $200 million to build four National Advanced Computing Centers to help stem the threat to our supremacy in supercomputers—because of stiff Japanese competition—let us be prudent to collaborate but not give away what may jeopardize the future position of U.S. industry. In all probability, the best way we can be sure of having a cutting edge in the coming competition for commercial fusion energy is to get our industry in a lead-position and our national laboratories and universities in supporting roles. We have little time to lose. Thank you.

[The prepared statement of Mr. Reichle follows:]

TESTIMONY

TO THE

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

OF THE

COMMITTEE ON SCIENCE AND TECHNOLOGY

OF THE

U.S. HOUSE OF REPRESENTATIVES

BY

LEONARD F REICHLE, GROUP V-P
EBASCO SERVICES INCORPORATED,
AN ENSERCH CORPORATION
MARCH 18, 1985

Madame Chairman and Members of the Subcommittee:

I am Len Reichle, Group Vice-President and member of the board of directors of Ebasco Services Incorporated, and Chairman of the Subcommittee's Magnetic Fusion Advisory Panel.

In general, I believe the state of the nation's Magnetic Fusion Energy Program is healthy, but not as healthy as it could be and should be. Ten years ago, the United States looked upon Fusion as our most attractive alternate energy option. At that time, we said to ourselves essentially what the DOE 1986 Congressional Submission Program Overview on Magnetic Fusion Energy says:

> "Fusion energy could have several attractive features including intrinsic safety and minimum environmental problems. Fusion would utilize a vast fuel reserve that is inexpensive, readily available, and free from threat of embargo." (p. 79, par. 1)

Ten years ago, despite our limited knowledge of the science and technology of fusion, we recognized fusion's great potential and we resolved to get on with the show. We planned a Tokamak Fusion Test Reactor (TFTR) to demonstrate "break-even" in the early 1980's. We planned an Engineering Device to become operational in about 1990. We planned a Demonstration Fusion Power Reactor for the Year 2000. With DOE funding and program support, the Princeton Plasma Physics Laboratory (PPPL) engaged Ebasco, who, along with Grumman Aerospace, our Team Subcontractor, designed and built the TFTR. The TFTR produced its first hydrogen plasma in 1982. The United States lead the world in the development of Fusion Energy.

Since then, the nation's Fusion Energy Program has departed from an action-oriented program. It has departed from a program with discernible "flagship" projects aimed at the goal of demonstrating fusion-generated electricity in the year 2000. It has become what DOE calls "...the nation's research effort to develop a major new energy source for this country in the next century." The program is less of an engineering-oriented program and more of a science-oriented program of the universities and the national laboratories.

Demonstration of "break-even" in TFTR has been delayed 2 years, but is now scheduled for 1986. On September 23, 1983, the Magnetic Fusion Advisory Committee (MFAC) recommended that a Tokamak Fusion Core Experiment (TFCX) be initiated. On February 3, 1984, the Energy Research Advisory Board (ERAB) recommended that DOE commit to building what it called a burning core Experiment - Basically the generic model of TFCX. Our national laboratories, universities and the fusion industry were in agreement to proceed with such a project. On February 14, 1984, PPPL issued a Request for Proposal (RFP) to assist PPPL in the design of a TFCX. Several strong industrial Teams spent hundreds of thousands of dollars on TFCX proposals. However, despite the strong support by the fusion community, on September 5, 1984, the RFP was withdrawn. Recently, on February 26, 1985, PPPL held an Information Meeting for a proposed ignition Studies Project. I hope such a "next-step" Tokamak project will be supported promptly by DOE and the Congress regardless of the funding-level of the over-all fusion budget.

As a nation, we have lost leadership already in steel-making, ship-building, radios and TV. In fact, in every major electronic market, including computer equipment, communications equipment, office machines, and semi-conductors and components, our imports are rising and our exports are falling. By failing sooner to have initiated the engineering design of a next-step Magnetic Confinement Fusion Device, we may already have assured our loss of leadership in Fusion Energy by 1990 or before. Let's do what we can to retain or regain leadership, even in a time of budget restraint.

What can we do? Madame Chairman, to suggest an answer to this question I shall address the three topics in your letter.

Topic No. 1 - The Requirements for Maintaining a Strong, Effective Fusion Program

Probably the most important requirement is the fostering of a perception by the U.S. Government and the public that it is important to develop Fusion energy promptly because it is potentially our best alternate energy option. it has the potential of producing commercial electric energy from inexhaustible fuel resources in a cost-competitive manner, safer and more environmentally compatible than coal, nuclear fission, and the fast breeder reactor.

So, our Advisory Panel, in its letter dated December 4, 1984 stated with respect to DOE's Magnetic Fusion Program Plan:

> "We appreciate the budgetary realism of the Plan, but we are distressed by the rate of progress envisioned for the near term."

Congressional Appropriations for the Fusion Program were approximately $468 million in FY 1984 and $437 million in FY 1985. The Administration's Request for FY 1986 is $390 million. This is a declining budget. But, a declining budget is no excuse for failing to proceed within the budget on a most-likely-to-succeed main-line effort, with only back-up support for alternate options. Typically, in an industrial R&D program, the "leading contender" among development options is fully funded, and other options and technology development are relegated to lesser levels of support within the limits of available R&D funds. Among fusion options, the Tokamak by consensus of most of the fusion community has a sound theoretical base, has world-wide experience behind it, and is the "leading contender". Only last week, Dr. Harold Furth announced at the ANS meeting in San Francisco that, with only 5 MW (out of a soon-to-be-installed 27 MW) of neutral beams, the TFTR had achieved temperatures as high as previously achieved, i.e. 70-80 Million degrees. Unofficially, I understand that it may have achieved more than 100 Million degrees.

Let us build on the progress we are making. Let us promptly take the next step. Let us begin a burning-plasma experiment. Our Advisory Panel, in its letter on December 4, stated:

> "We believe that, even within current budgetary restraints, it should be possible to undertake an affordable burning-plasma experiment by modifying existing facilities with that objective."

Also, our Advisory Panel, recognizing the need for balance in the Program, said:

> "We are pleased that the Plan recognizes the need to address materials and nuclear technology issues, as well as the identification of an optimum confinement system."

Topic No. 2 - The Potential for Building a Low-Cost Ignition Device

Our Advisory Panel, on December 4, stated:

> "A conceptual design for such a project should be prepared as soon as possible, aimed at a capital cost below $500m and a project starting date in FY 87-88. The start of construction should be contingent on the demonstration, during 1986, of high-temperature plasma confinement results in TFTR that prove the feasibility of achieving ignition in such a device."

PPPL, on February 26, 1985, at its Information Meeting on the Ignition Studies Project suggested that an Ignition Device could be built for $300-400 million if facilities and equipment at the TFTR complex are utilized. Ebasco has not made an independent estimate, but believes that, if project management, engineering design, procurement, installation and assembly of the device is assigned to acknowledgeable industrial subcontractor or industrial team, the probability is high that such a device can be built for less than $400 million.

Certainly, if the will to undertake such a project exists in the DOE, even a budget limited to $390 million per year could fund the project. Of course, some other programs would have to be curtailed. That would be a small price to pay for retaining or regaining world leadership in Fusion Energy.

Topic No. 3 - Continuing Role of Industry in the Fusion Program

In December, our Advisory Panel stated:

> "We believe that meaningful involvement of industry is essential to the healthy evolution of the United States fusion program and should be made explicit in the Plan."

I regret to report that industrial involvement in the nation's Fusion Energy Program has become less rather than more meaningful. Industrial subcontract work is being reduced as DOE-funded budgets at National Laboratories and Universities are reduced. But, what is of even greater importance is how our industry is involved. For conceptual design of the TFCX, PPPL proposed to "mix and match" various pieces of industrial proposals regarding the TFCX, and to manage and direct the engineering design themselves rather than to engage one of the industrial Teams to perform the design, procurement and installation as a responsible industrial subcontractor. If the United States is to compete with Hitachi in Japan, we would be well advised to have a U.S. industrial company or industrial team in the responsible lead-role. That is how the TFTR was designed and built.

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> "We support the strategy of seeking international collaboration on major fusion projects and believe that the initiation of the proposed burning plasma experiment will help to make the united states a strong partner."

Such international collaboration can be successful in the early programmatic stages of scientific research and development. However, as the program proceeds to an engineering stage, with proprietary interests in systems engineering and hardware, intergovernment collaboration inevitably becomes less effective.

Let us not permit international collaboration to be a substitute for strong U.S. Government support of our national Fusion Energy Program. At a time when the U.S. National Science Foundation is spending $200 million to build four National Advanced Computing Centers to help stem the threat to our supremacy in supercomputers - because of stiff Japanese competition - let us be prudent to collaborate but not give away what may jeopardize the future position of U.S. industry. In all probability, the best way we can be sure of having a cutting edge in the coming competition for commercial fusion energy is to get our industry in a lead-position and our national laboratories and universities in supporting roles. We have little time to lose.

Mrs. Lloyd. Thank you, Len. Thanks to all of you for very outstanding statements.

The program has been reduced from the 1985 funding to a level of about $390 million. This would certainly take care of a significant effort. It seems to me that the program is threatened by these annual reductions and it was well put in your statement, from your December meeting, that the administration is trying to substitute international cooperation for a full commitment to moving ahead with fusion energy. I certainly agree that we do need a policy statement on our goals as well as a plan for meeting our goals. I share your fears that we can arrive at a level of funding that would lead to mediocrity. At that point, our fusion program would be rather pointless.

What could be done, Mr. Reichle, to consolidate or streamline our fusion research and development to reduce costs without losing this momentum that we need, and at the same time, shorten the time for a test reactor? What would you like to see done?

Mr. Reichle. It is a very difficult problem because every organization, laboratory, university, and industrial group that is doing something depending upon government funding for it would sooner continue doing what it is doing to a further resolution of whatever the problem is. Somebody has to go in and set priorities. We do have a mainline "flagship" program and other things that do not support that, just have to be put in a second position. In the g pattern of industrial research and development, you pi the "leading contender" and back it fully and even in a declining budget you are still keeping a mainline effort going. That doesn't mean you're going to give up the supporting programs; you may just have to cut back on some of the technology development work and put that off for a future period. That might even be good.

Technology development for its own sake is often not too helpful. During the period of the development of the fission program and the breeder reactor program, we spent millions and millions of dollars on technology development which never got applied because we didn't get behind the Clinch River Breeder and drive that through. If we could get behind a flagship program in magnetic fusion and orient technology development to support that, I would think we would be closer to achieving our national objective.

Mrs. Lloyd. Is your panel going to make such recommendations? In other words, like we say in Tennessee cut bait and start fishing?

Mr. Reichle. I will be glad to convene them again and ask the opinion of the panel as a whole.

Mrs. Lloyd. Thank you. Dr. Ohkawa, certainly scientific excellence is necessary if we are going to have a strong fusion program. Other nations are going to see a little interest or a little point in reaching agreement with us and working on our fusion programs. Will there be any synergism between our TFTR and big D work when big D goes back on line? Would you describe this for us, please sir.

Dr. Ohkawa. From the beginning, the two machines are complementary. The TFTR is a high field, very big machine with the capability of D-T experiments. D-III was designed to have more of the high-beta shaped plasma. In that sense, that field of investigation and science would be complementary. Does that answer your question?

Mrs. **Lloyd**. Thank you, very much. I have a question for Dr. Dean, as well as Mr. Reichle. Does our present budget give us anything near a strong industrial base? You brought out in your testimony that we needed to bring on more incentives for industrial participation. In this scenario, can we look for further industrial participation and, if not, what can we do within our current funding levels to correct this problem. If we were able to add $15 or $25 million on to the program, what would we do then?

Dr. **Dean**. I think that industrial involvement and participation in this program is absolutely essential if you expect some type of a commercial product to come out of this.

Mrs. **Lloyd**. Can we do it within this present budget?

Dr. **Dean**. You can do it within this present budget but it takes very active, aggressive management action to identify those things in the program that can and should be done by industry and not to do them in the laboratories and not to do what is natural in the laboratories. There's a lot of things that are going to be done for that money. A lot of that work can and should be done by industry. The industry is there. We need to deliberately identify work packages and put the work out on the street and let industry bid for it.

Mrs. **Lloyd**. Len.

Mr. **Reichle**. I believe there's a matter of perception of what is meant really by industry involvement. Often it is thought that industry really shouldn't be involved unless they can come in on a commercial profitmaking basis. When we were about the problem 30 years ago of developing a nuclear submarine, we didn't ask whether General Electric or Westinghouse would come in because they could make profit at it. We wanted to bring them in as a nation because they were the best industrial organizations to do the job. I think in the same way today we want to bring in industrial companies in the engineering phase, design, construction, installation, and assembly for the things they can do best while making sure we have ample funding for the universities and the laboratories in what they can do best.

Mrs. **Lloyd**. How would you suggest that we allocate additional levels of funding for successful——

Mr. **Reichle**. I would suggest that when we get to the stage of a conceptual design of a new device that we ought to put that out on the street for industrial competition and for groups to assemble and bid to do the job. The universities and laboratories shouldn't be involved in that but should concentrate on what they know best. They should then work hand-in-glove together.

Dr. **Dean**. You're probably aware that last year, when we thought we were going to get TFCX off the ground, Princeton had an RFP out; they were going to bring in several teams of industries. The industries had put their bid packages together; they spent a lot of company money. Those RFP's ought to get back out on the street and this money ought to be put out to these industrial teams and let them do the conceptual design on this ignition experiment.

I also think industry ought to participate in this enhanced systems studies work for the longrange by looking at what does it take to make a commercial reactor and what kind of a development program ought to be in place. I think industry ought to be involved in those commercial reactor design studies, also.

Mrs. LLOYD. Do you think we should bring the Government witnesses back in for further hearings in this direction?

Dr. DEAN. They're all my friends. I——

Mrs. LLOYD. They're my friends, too. I just want to get on with the show.

Dr. DEAN. It's a management problem. We've got to get on with the job. You asked Mr. Reichle how we could get this program moving at $390 million. If you look back the past 5 years, the program has spent about $400 million a year and one of the reasons we don't have more for our money than what we've got is because we can't decide to make commitments to go off and do the next thing. Every year we spend all this money and every year we come back and we redesign. You've got to bite the bullet and get on with the job. If you do, you're going to get a lot more for your money 5 years from now than if you just keep delaying decisions.

Mrs. LLOYD. Until you're going to have more response from the Congress as well; when we can come with something in hand and say, "Look, this is what we're doing with your money, and this is the prospect for future growth." When we go on and on and never reach a decision, we see that our support in the Congress begins to dissipate when there is such a hard competition for our Federal dollars.

Dr. Parker, what low-cost devices do you feel would be appropriate for a sustaining ignition?

Dr. PARKER. I think in my testimony I indicated that the device should be a tokamak and I believe it should be very high-field, high relative to the large tokamaks of today, for example, JET or TFTR. I think it should be not more than two meters in major radius, which is small.

Mrs. LLOYD. A small machine with high magnetic fields?

Dr. PARKER. A small machine with high magnetic fields, that's correct.

Mrs. LLOYD. The existing ATF facilities at Oak Ridge is really not attendant and it cannot achieve ignition. This was not the purpose of it. What is your guess as the possibility of using a second generation ATF of achieving low-cost ignition in the next decade?

Dr. PARKER. I think it will take at least a decade to learn how to design such an advanced facility for ignition and I think that if we hold off and wait for further development of an advanced concept, such as the ATF device, that we'll miss an opportunity that we could take now with the tokamak as the vehicle. My recommendation is that we not wait for the development of an ATF concept, no matter how many advantages we might see in the long run from the ATF, but get on with ignition now. Having that behind us, fold that into the next generation ATF device.

Mrs. LLOYD. Thank you. I think we all agree that ignition is the major goal for our fusion program and we need a significant experiment to verify this device.

The administration continues to push for international cooperation. I think that the real challenge we have right now is adopting a near-term goal for a low-cost device under $500 million so as to go forward.

Dr. Dean, if we cooperate with other nations on the fabrication of a large ignition device for the distant future, what would be the parameters of such a device? What would you want for the primary feature of the components? What is the advantage if we look for our near-term goals plus our longer-term international cooperation?

Dr. **Dean**. This next device has got to make significant amounts of fusion power regularly and reliably both for physics and for experience. It would provide the basis on which to design something that would be more commercially viable. I would like to endorse the version of Dr. Ohkawa's speech on international collaboration which is that we ought not to allow this thing to be done simply by a bunch of State Department-type negotiators trying to form some international committee to design this machine.

We ought to decide to do it and we ought to go about designing it. We ought to invite other countries in to be participants so that they buy in but that we don't wait around for some committee to finally—then it would take years. But I think we can decide to do it now, and if we did, I think other countries would come in.

Mrs. **Lloyd**. We would go ahead and take the lead and assign the responsibilities and the division of work among the contributing partners.

Dr. **Dean**. We could negotiate what they were buying in for. We ought to take the lead, I think and be in control of this thing and push it.

Mrs. **Lloyd**. Thank you. Dr. Ohkawa, what is DOE's involvement in the OHTE?

Dr. **Ohkawa**. Right now we have a very small involvement with DOE in letting us use their power equipment. Right now we are discussing with DOE for the second step to make an industry-Government joint program. We will be discussing this thing in the coming months as to what can be done as an industry-Government joint program.

Mrs. **Lloyd**. So you just began the negotiations on it?

Dr. **Ohkawa**. Yes.

Mrs. **Lloyd**. What would the design parameters of a fusion device have to be to sustain ignition?

Dr. **Ohkawa**. For the OHTE approach? It would be similar to the tokamak in a sense. You have to have several million ampere of the current in a torus. Compared to tokamak, these things work much higher plasma density or pressure. Physical size of the magnetic field would be somewhat smaller. It would be a very interesting comparison to tokamak and the phase 2 objective is to bring this kind of approach to the same kind of status tokamak is now.

Mrs. **Lloyd**. Would you describe for the record the difference between OHTE and the ATF.

Dr. **Ohkawa**. The ATF is what we call a more traditional toroidal approach, where the magnetic field lines would go around the torus more toroidally, and the OHTE, the magnetic field line goes more like a tight spiral. In that sense, the difference is in the magnetic geometry.

Mrs. **Lloyd**. Thank you very much. Mr. Fawell.

Mr. **Fawell**. This will be directed to anyone of you. There seems to be a consensus among you that this country should pursue development of an ignition experiment to obtain burning plasma. Do you all have any ideas as to where such an experiment should be sited to maximize the R&D results and minimize the costs?

Dr. Parker, would you want to respond?

Dr. **Parker**. I think the panel would agree and I would urge that we have to identify the resources necessary for the ignition machine and site the device wherever the existing resources best match the needs of the experiment. I don't think I would like to presuppose where that might be. I think there are a number of areas in the country that could be considered. I think we would have to look at each one and decide where the resources are best matched. That's a bit of a hedge, I'm afraid.

Mr. **Reichle**. I was going to suggest that Princeton is a very likely candidate because of the facilities of the TFTR, but I agree that a careful study should be made as to the requirements of the new experiment and other candidate sites.

Dr. **Dean**. Without doing a careful analysis, you can't know this answer for sure. It would appear, from first principles, that it would have to go at an existing Federal facility, of which there are a number of national laboratories that could be the place where this could go. Beyond that, I don't think you could really pick right off the top of your head, which would be best.

Mr. **Fawell**. Do you believe that other countries, particularly the Europeans and the Japanese, are willing to come forward to share the cost of developing such an ignition experiment, Dr. Dean?

Dr. **Dean**. I believe absolutely, but there has to be leadership for it. I don't believe this will emerge from some kind of a committee of international negotiators. I believe somebody has to really want to do it. If somebody's really serious about doing it, other people in other countries will want so badly because they won't have access to any other way and they'll see it as a cheap way of gaining experience. I think other countries will buy in only if we're determined to do it. If other countries take the lead and start doing it themselves, I think we would buy in. I don't see that happening at the moment.

Mr. **Fawell**. Dr. Dean, I'm a newcomer to this subcommittee and to the Congress, but I've sat here and heard a number of people who are interested in energy R&D talk about the decline of the civilian-oriented R&D and the rise of R&D in regard to defense weaponry or defense, in general. Certainly, I can understand the need for more R&D in that area. In your opinion, is there any sound reason for this decline or what are the reasons for this? Is it simply because of the deficit? We just don't have money and we have to make these choices or is there a sound reason for perhaps tending to give more of this to private industry on the belief that we haven't been motivating them to do all they ought to do, and so forth. I'm concerned about it. I think it's lamentable to see this slowly occurring and I think you are the one that pointed out that you also had a great deal of concern about it.

Dr. **Dean**. I think it's unfortunate that in our society we seem to have to go to extremes. In the 1970's, we had the time that there was this terrible energy crisis; that the whole world is going to run out of everything in 10 years and we had to go crash and do everything. Then a few years later we decided that was totally wrong; That there's an energy glut and there's no energy problem at all. Now we have to swing all the way back the other way and do nothing. I don't know why we can't do something on some kind of even keel which recognizes that we do have a long-range problem; that it's not in the interest of the private sector to fund things that pay off in 20 years; that the Government has some kind of responsibility to the country to do these kind of things and it can't get away, in a society that's as technologically oriented as this, with just doing basic research.

I think the administration has just swung too far the other way, and somebody's got to bring them back. Maybe in the next administration they will swing back around to even keel, but it would be nice if we didn't have to wait another 4 years, or wait around until there's another crisis.

The energy predictions that I've seen for electricity, even on modest growth, indicate that there's going to be a real problem of how many powerplants have to be built early in the next century. We've got to have technologies to do that. I don't see any plan to mine and transport all this coal and get rid of all the ash and solve the CO problem. It's not obvious to me that coal is the best and the big winner. There are a lot of technologies here that have to come along. I don't think it's right that the Government won't support them. If the Government can't afford it, then there ought to be some way of making it attractive to the private sector to do it. The private sector doesn't have the motivation for now.

Mr. **Fawell**. Thank you.

Mrs. **Lloyd**. I would like to apologize to the ranking Republican on the full committee, Manny Lujan. We certainly did not intentionally overlook you.

Mr. **Lujan**. Mr. Fawell was here before I was. That's the proper order.

Mr. **Fawell**. I would like to extend my apologies, too.

Mr. **Lujan**. There's no need for that.

Mrs. **Lloyd**. We do welcome you to the hearing.

Mr. **Lujan**. Let me see if I understand; 2 or 3 years ago, we were talking about a $2 or $3 billion experiment. Are we still talking about the same thing for $400 million?

Dr. **Ohkawa**. No, sir. The simple answer is "No."

Mrs. **Lloyd**. Mr. Reagan didn't do that, Mr. Lujan.

Mr. **Lujan**. It seems to me the same end result is that not correct? It's an ignition experiment and we were talking about a larger one, or am I completely off?

Dr. **Parker**. We basically cut back somewhat on the objectives of the experiment and to the degree to which the experiment might be prototypical of a reactor. I think we've given up on those two areas in order to cut perhaps to the heart of the matter which is the most fundamental question about ignition and how the heat is confined by the burning plasma.

Mr. **Lujan**. What were we going to do for $2 or $3 billion other than ignition?

Dr. PARKER. We would have had ignition for what we call an equilibrium situation, that is one that would not be evolving with time. It's likely in the present thinking that the small ignition experiment will just be the initial few frames, if you will, of a scenario which will be unfolding with time. We will not be able to see the rest of those frames without building a larger machine.

Mr. LUJAN. Then if we follow that, this one should be built by the United States and perhaps for the expansion would be the international cooperation. Is that right?

Dr. PARKER. I think that's a scenario that is certainly appearing to many people. When we get to the $2 to $3 billion stage, that would be the appropriate time for a real partnership between the European and Japanese and United States communities for that next larger phase.

Mr. LUJAN. Would you expand this facility or built a completely new one?

Dr. PARKER. I think that depends on the answer to Mr. Fawell's question which is where this facility might be sited. That might be a consideration in the siting whether or not to build the inexpensive machine at an existing facility which can't be expandable, or whether to develop the machine at a facility that might have that potential for future expansion.

Mr. LUJAN. In the international discussions, that would be part of the discussions, wouldn't it, as to where it would be built, whether in this country or in another country.

Dr. PARKER. One could see arguing for a decade about that.

Mr. LUJAN. We would preempt then where the larger machine was going to be if we put this smaller——

Dr. PARKER. This could be an attractive feature of having such a site available and it would tend to draw it naturally to the United States——

Mr. LUJAN. Attractive to us—unattractive to whoever we could convince to be our partners, perhaps. I'm concerned about one statement, Mr. Reichle, about picking out the mainline, or flagship, method that we will follow even if it means cutting out alternative ways of achieving the same end. Are there not those that believe that the Tokamak will not, in the final analysis, be the solution to the fusion problem? Is there consensus on everyone's part——

Mr. REICHLE. That is true, there isn't complete agreement. I think there is general agreement within Western Europe, Japan, and the United States that the Tokamak technology has sound theory behind it and has achieved demonstrable results that are proceeding up an orderly scale.

I didn't mean to imply that we should stop all alternate options. I was trying to say that in a period of budget restraint, it would be desirable still to go ahead making certain that our leading contender was fully funded to the next phase.

As you just pointed out, we have cut back from a $2 billion or more program to a $400 million program just to get the next step achieved, that of ignition. We have the spark; what we need is the fire in the plasma. As a matter of fact, when we achieve ignition with the additional $400 million, we may be thinking collectively as a fusion community, about a configuration of the next step beyond that which we don't conceive right now. If everything went perfectly, we might even go from there directly to a power demonstration plant. If the review of the community thinks that's too big a step, then we probably will want a smaller jump in technology and would be guided according to results of the ignition experiment.

The idea is not to give up options but to cut them back also to perhaps a level less than we would like, but still not to do away with it.

Mr. **Lujan**. One of the concerns that I have in not pursuing some of the other options is in the fission business we find ourselves with a great big problem. I guess we've come to the realization that those big, big powerplants have to have something done about them because we just won't be building anymore, unless we do either modular or smaller ones. Are we heading in the same direction with fusion if we do put all our eggs into the Tokamak basket? Will nobody build them because they're going to be so expensive and they will then be abandoned much as we've abandoned the present powerplants?

Mr. **Reichle**. I think on that score there is not unanimity of opinion. Many of the people in the present fission community, among both the utilities and manufacturers engineer companies, are saying that they're still going to have their major central station power supplied by very large reactors. It's a question whether they're 1,300 megawatts or maybe 9 or 10. A 900-megawatt plant is still a very big one. On the other hand, there are those who say we must go to much smaller plants—200 or 300 megawatts. Perhaps the economics will be a determining factor. It's been economics that has been the motivation for a larger plant. If technology can come up with a very small unit that can be competitive with a very large one, then I think it will have won the race.

I think the probability is against that but that's not certain.

Mr. **Lujan**. Thank you, Madam Chairman.

Mrs. **Lloyd**. Thank you very much, Mr. Lujan. Dr. Dean, I will have to agree with you. It looks as though the administration has taken a roundabout approach to dismantling the Department of Energy. It was well put.

Thank you, gentlemen.

Our next witness is Mr. Joseph Gavin, of the Grumman Corp. and headed the National Research Council's Engineering Board Committee on International Cooperation in Magnetic Fusion. Mr. Gavin, this topic has gotten a lot of attention and I've been concerned that there has been a little too much wishful thinking by the administration on this issue, so I am looking forward to your realistic assessment. Your entire written statement will be included in the record; you may summarize or proceed as you wish. I would also like to tell you at this time that your fine people at your Long Island facilities were very hospitable. Dr. Dugan and I certainly enjoyed our trip to your facilities on Long Island. It was nice to meet your secretary and your associates. We were given the red carpet treatment. We thank you for that.

STATEMENT OF JOSEPH GAVIN, CHAIRMAN OF THE EXECUTIVE COMMITTEE, GRUMMAN CORP., COMMITTEE ON INTERNATIONAL COOPERATION IN MAGNETIC FUSION, NATIONAL RESEARCH COUNCIL

Mr. GAVIN. Thank you, Madam Chairman. It's a pleasure to be here today and to speak on behalf of the committee which I had both the honor and the privilege of chairing.

I, too, tend to agree with you that there has been, perhaps, some wishful thinking about international collaboration. Rather than go through the entire statement which I prepared, I would like to emphasize some of the key points from it.

As you know, we concluded that in the balance international collaboration was a preferable course of action. Unfortunately, it is not going to happen quickly and it's not entirely clear that the people we might collaborate with indeed wish to collaborate with us. I think it's worth exploring these matters in a little bit of detail.

To go to the heart of the matter, it seems to me that we're talking about priorities, about attitudes, and about perceptions. The other gentlemen have talked to you about the parameters of the device; I'm not going to mention a single parameter. I would like to concentrate on the expressions of opinion which we encountered and which we tried very hard to report accurately.

Fundamentally, it seems to me that this country has basically three choices. We can decide to make the necessary commitment to become the continuing all-out competitive leader in all aspects in the development of fusion energy. This is an expensive course. Compared with some of the other national undertakings, it may not be anymore expensive but it is surely the most expensive of the three I am going to mention.

The other alternative is to withdraw from this arena completely. This is the most economical approach—economical in the short term—perhaps not economical in the long run, with the idea that in the future we would purchase the developed technology from others. We have muddled our way into that position with regard to the breeder reactor. I'm not here to say that the breeder reactor is vital to our future, but if it is vital to our future, it appears to me the chances are that we're going to buy it from the Europeans, as it stands today.

In between those two extremes there is the potential for engaging in widescale international collaboration in the sense that by collaborating it may be possible to bring about a more orderly and a more assured development of fusion energy. Certainly if one looks at the global totality of the undertaking, it doesn't make sense in the long run for there to be three or four of every major experiment duplicated in various parts of this world.

I'd like to go into the question of where people stand or where they think they stand. I think it's more important as to where they think they stand. In comparison with the United States, it was perceived by our committee that the Europeans believe that they are on the threshold of assuming world leadership. Indeed, they have a well organized program. The accomplishments with the joint European torus as well as the supporting undertakings—there is a program, it's laid out in detail—everyone is willing to concede that the finance ministers may modulate the rapidity with which this program proceeds and the program may get changed. As of today, one can go and find a very detailed complete program for the development of fusion energy.

I am emphasizing this because, by comparison, in their eyes, the Europeans do not see that the United States has a similar convincing program laid out. At this point, it's not clear to me whether the Europeans would necessarily respond with great enthusiasm to a bid to collaborate. As a matter of fact, as an indication of where their confidence level stands, they have already begun preliminary work on designing the next European torus. This presumably will not really be undertaken for another 5 years or so, but at least the preliminary work is going ahead on the basis that the results from the joint European torus will indeed provide the useful starting point for the next step. This is best described as a reactor level device, perhaps a demonstration level device.

The Japanese program is distinguished by comparison with both the Europeans and the Americans with a singleness of purpose. Bluntly put, there is no indigenous energy sources to speak of. We were told that they are going to make fission work and fusion work. They are now bringing the JT-60 Tokamak to completion. It is supposed to operate during the coming year. While it is not as ambitious in some respects as the TFTR, it will not use the deuterium-tritium combination, nevertheless it is a world class Tokamak. It is one step toward the development of fusion energy.

They also are very clear in pointing out that there will be a fusion experimental reactor in the middle to late 1990's, where those objectives would be very similar to the next European torus.

I think that it should also be said that the Japanese undertaking differs from the others that we examined in the distinctly deep involvement of industry. There's no question but what Japanese industry has a headstart in understanding how to design and build the components involved in a fusion reactor.

I should say also that we did not investigate what was going on in the Soviet Union. Perhaps this would be something worth contemplating as being useful in the future.

We have different programs. We have different priorities. We have different perceived momentum. I think that the American program suffers in the eyes of the people overseas as having peaked out and perhaps losing momentum. I'm a little sensitive on this subject because during part of the tour involved with this committee, we learned that the TFCX had been set aside as a useful project. Consequently, our discussions took a different turn at that point. I think that this confirmed again that this country does not have a well thought out program that stands still. There's a neat balance between having a program that's cast in concrete and one that is agile and able to take advantage of new developments as they develop. Certainly there is a place for having laid out as well as we know how to do it, the program that is intended for the future. I think that I will come back to that question of the type of planning and the details that are involved.

We did say that there is an opportunity now for developing large-scale collaboration and we said this because of the status that these other programs have and where it's obvious that it will be sometime before the next really major undertakings occur. Here is a chance to put our programs together to see wherein they either disagree or agree and from that, determine whether there are parts that make sense for a joint undertaking.

I have mentioned already that the United States is perceived as lacking in a firm commitment and a realistic plan. This is by comparison. These other areas do have a very detailed and very well thought out plans. We might not agree with them precisely, but you must look at it through their eyes. They perceive themselves as having a firmly committed research program. I might also point out that in areas having not to do with neither fusion nor the Department of Energy, the United States has been perceived at times in the past as being an unreliable partner because of unilateral action in either slowing down or withdrawing from a joint project. Some of this could be construed as bargaining toward the first conversations with respect to collaboration. It is a problem and I'm sure that any agreement to undertake international collaboration and fusion is going to require something very firm in the way of a commitment. Something that is long term in the way of a commitment. These are things that we do not do from year to year very readily.

I think that there are obviously involved in international collaboration many considerations that remain to be examined and worked out. These range all the way from the question of patent rights to the question of what is a government subsidy. In these various governments, different research programs of this nature are subsidized in different ways so that it's not easy to match them up and say that we understand that the subsidy that is occurring in a European endeavor is similar to or different from the subsidy that this government might apply to a similar endeavor here.

Because of these differences, it becomes very difficult to draw the line between what is government support and what is industry support. What is a fair share? If we invite somebody to join us in a particular project, it's going to take a considerable amount of time to determine how much is each party really putting up.

One of the things which enters into this and was alluded to by one of the earlier speakers is the fact that I think both the Europeans and the Japanese have become rather more motivated to support certain lines of research and development in order to seize a future commercial advantage. I think we have tended not to understand the degree to which government and industry aims are so closely woven together in these other places.

I think that in looking at all of these detailed considerations, we conclude that surely we can work these out. This is particularly true in the next stages where we will still be in the research and development stage of fusion and we will not yet be at the commercial power reactor stage of development. I would like to finish by highlighting once again the recommendations that the committee made with regard to international collaboration.

The first priority should be the establishment of a clear set of policies and objectives and a considered program plan for future U.S. fusion activities. Then, having accomplished that, the United States should take the lead in consulting with perspective partners to initiate joint planning, which is a prerequisite for any large scale collaboration, and for any small scale collaboration.

When this report was written, our attention was focused largely on relatively more expensive experimental undertakings. It is quite possible that some initial international collaboration on a less expensive device would be easier to define and to come to agreement on.

That concludes my statement, Madam Chairman. I would be happy to answer any questions.

[The prepared statement of Mr. Gavin follows:]

INTRODUCTION

MADAM CHAIRMAN AND MEMBERS OF THE SUBCOMMITTEE, I AM JOSEPH GAVIN, CHAIRMAN OF THE EXECUTIVE COMMITTEE, GRUMMAN CORPORATION. I AM PLEASED TO TESTIFY BEFORE YOU TODAY ON INTERNATIONAL COOPERATION OF MAGNETIC FUSION ENERGY.

BECAUSE THE MAGNETIC FUSION PROCESS HOLDS UNIQUE PROMISE AS A LONG-TERM ENERGY SOURCE, EFFORTS HAVE PERSISTED FOR MANY YEARS TO SOLVE ITS CHALLENGING SCIENTIFIC AND ENGINEERING PROBLEMS. MAJOR PROGRAMS HAVE BEEN UNDERTAKEN BY THE UNITED STATES, EUROPE, JAPAN AND THE SOVIET UNION. AS THE SIZE AND COMPLEXITY OF THE EXPERIMENTAL DEVICES HAVE GROWN, INTERNATIONAL COOPERATION HAS OCCURRED IN ORDER TO PRODUCE EARLIER RESULTS, TO SHARE RISK, TO MINIMIZE INVESTMENT OR TO ACQUIRE SKILLS. FACED WITH EVEN MORE DEMANDING FUTURE PROGRAM REQUIREMENTS, OFFICIALS OF THE U.S. DEPARTMENT OF ENERGY ARE CONSIDERING WHETHER GREATER LEVELS OF INTERNATIONAL COOPERATION IN MAGNETIC FUSION IS DESIRABLE.

IN SEPTEMBER 1983, I WAS ASKED BY THE NATIONAL RESEARCH COUNCIL TO CHAIR THE COMMITTEE ON INTERNATIONAL COOPERATION IN MAGNETIC FUSION ENERGY. THE COMMITTEE CONSISTED OF TEN MEMBERS WITH BROAD BACKGROUNDS IN ELECTRICAL ENGINEERING, PLASMA PHYSICS, FUSION TECHNOLOGY, FUSION REACTOR DESIGN, INDUSTRIAL PARTICIPATION IN HIGH TECHNOLOGY PROJECTS, ENERGY SUPPLY, TECHNOLOGY TRANSFER, AND THE LEGAL, DIPLOMATIC AND POLITICAL ASPECTS OF INTERNATIONAL GOVERNMENTAL VENTURES.

THE PURPOSE OF THE COMMITTEE WAS TO STUDY AND RECOMMEND A WORTHWHILE COURSE OF ACTION IN INTERNATIONAL COOPERATION AS MEASURED BY THE CRITERIA OF ACCEPTABLE POLICY, TECHNICAL MERIT, AND PRACTICAL WORKABILITY.

TO ACCOMPLISH THIS, THE COMMITTEE:

A. IDENTIFIED AND ADDRESSED THE MOST IMPORTANT ISSUES IN INTERNATIONAL COOPERATION IN MAGNETIC FUSION ENERGY.

B. REVIEWED AND DISCUSSED ALTERNATIVE AVENUES OF COOPERATION IN VIEW OF SCIENTIFIC, TECHNOLOGICAL, AND ENGINEERING NEEDS OF FUSION POWER.

C. REVIEWED U.S. GOALS AND OBJECTIVES FOR THE DEVELOPMENT OF MAGNETIC FUSION AS THEY MAY BE PHASED OVER TIME AND AS THEY RELATE TO TECHNOLOGICAL PROGRESS, INDUSTRIAL INVOLVEMENT AND SELECTED SOCIO-ECONOMIC FACTORS.

D. IDENTIFIED AND CHARACTERIZED LONG-TERM IMPLICATIONS OF VARIOUS AVENUES OF INTERNATIONAL COOPERATION WITH RESPECT TO U.S. GOALS.

E. OBTAINED THE VIEWS OF LEADERS OF THE U.S. AND FOREIGN FUSION COMMUNITIES ON THE MATTER OF BENEFITS ALREADY REALIZED FROM INTERNATIONAL COOPERATION IN MAGNETIC FUSION ENERGY AND BENEFITS EXPECTED FROM ENLARGED COOPERATION.

F. RECOMMENDED, WITH LIMITATIONS, AVENUES OF FUTURE INTERNATIONAL COOPERATION.

THE COMMITTEE ALSO RELIED UPON INFORMATION OBTAINED THROUGH VARIOUS MEETINGS WITH SELECTED MEMBERS OF THE DEPARTMENT OF DEFENSE, THE NATIONAL AERONAUTICS & SPACE ADMINISTRATION, AND OTHER GOVERNMENT AGENCIES WITH EXPERIENCE IN INTERNATIONAL COOPERATION.

DISCUSSION

THE FOUR MAJOR MAGNETIC FUSION PROGRAMS OF THE WORLD, THE U.S., EC, JAPAN AND USSR, ARE OF COMPARABLE MAGNITUDE AND ARE AT A COMPARABLE STAGE OF DEVELOPMENT. IN EACH OF THESE PROGRAMS IS A "SCIENTIFIC FEASIBILITY" EXPERIMENT BASED ON THE MOST ADVANCED MAGNETIC CONFINEMENT CONCEPT, THE TOKAMAK. THE U.S. AND EUROPEAN COMMUNITY (E.C.) PROGRAMS RECENTLY STARTED OPERATING; WITHIN THE NEXT YEAR OR TWO, PROGRAMS WILL BEGIN OPERATION IN JAPAN AND USSR RESPECTIVELY.

BROADLY SPEAKING, THE NEAR-TERM TECHNICAL OBJECTIVES OF PROGRAM PLANNERS IN THE FOUR PROGRAMS ARE SIMILAR:

1) TO MAINTAIN A VIGOROUS SCIENTIFIC BASE PROGRAM

2) TO INITIATE A MAJOR "NEXT-STEP" TOKAMAK EXPERIMENT

3) TO CONTINUE TO DEVELOP THE LESS MATURE ALTERNATIVE MAGNETIC CONFINEMENT CONCEPTS

4) TO EXPAND THE FUSION TECHNOLOGY DEVELOPMENT PROGRAM

PURSUIT OF THESE OBJECTIVES IS FINANCIALLY CONSTRAINED, IN VARIOUS DEGREES, IN EACH OF THE FOUR PROGRAMS.

THE PHYSICS OF LABORATORY PLASMAS AT NEAR FUSION CONDITIONS IS ESSENTIALLY AN EXPERIMENTAL SCIENCE TODAY. WORLD LEADERSHIP IN FUSION GENERALLY RESIDES IN THAT COUNTRY POSSESSING THE EXPERIMENTAL FACILITIES WITH THE GREATEST CAPABILITY TO EXPLORE THE FRONTIERS OF PLASMA PHYSICS.

ALTHOUGH YOU ARE WELL ACQUAINTED WITH THE U.S. FUSION ENERGY PROGRAM, I WILL COMMENT ON IT TO ESTABLISH CONSISTENCY IN PRESENTATION. THE U.S. HAS A STRONG EXPERIMENTAL TOKAMAK PROGRAM THAT HAS ESTABLISHED MANY OF THE WORLD RECORD PLASMA PHYSICS PARAMETERS. TWO OF THESE EXPERIMENTS, TOKAMAK FUSION TEST REACTOR (TFTR) AND DOUBLET III, SHOULD CONTINUE TO EXTEND THE KNOWLEDGE OF PLASMA PHYSICS FOR THE NEXT FIVE YEARS OR SO. THE U.S. ALSO HAS THE LEADING EXPERIMENTAL PROGRAM IN THE TANDEM MIRROR CONFINEMENT CONCEPT WHICH IS THE MOST ADVANCED ALTERNATIVE CONCEPT. SMALLER PROGRAMS ARE GOING FORWARD IN OTHER LESS ADVANCED ALTERNATIVE MAGNETIC CONFINEMENT CONCEPTS.

THE EC PROGRAM IS PERCEIVED BY ITS PARTICIPANTS TO BE ON THE THRESHOLD OF ASSUMING WORLD LEADERSHIP IN FUSION. THIS IS BASED ON A NEW GENERATION OF TOKAMAK EXPERIMENTS COMMONLY KNOWN AS JET, TORE SUPRA, ASDEX-4 AND FTU THAT WILL BE OPERATING OVER THE NEXT DECADE. THIS VIEW IS SHARED BY MANY IN THE U.S. THE EC PROGRAM MANAGERS BELIEVE THAT THEY SHOULD MAINTAIN THEIR PROGRESS TOWARD LEADERSHIP BY CONSTRUCTING A MAJOR NEW TOKAMAK EXPERIMENT, NEXT EUROPEAN

TORUS (NET), TO OPERATE IN THE MID TO LATE 1990'S. NET HAS THE PHYSICS OBJECTIVES OF ACHIEVING AN IGNITED PLASMA AND A LONG-BURN PULSE AND OTHER AMBITIOUS TECHNOLOGICAL OBJECTIVES. PLANNING AND CONCEPTUAL DESIGN WORK ON NET HAS BEEN AUTHORIZED BY THE COUNCIL OF MINISTERS OF THE EUROPEAN COMMUNITY AND INITIATED AT THE TECHNICAL LEVEL. THE DECISIONS WHETHER OR NOT TO PROCEED TO ENGINEERING DESIGN AND TO CONSTRUCTION WILL BE MADE IN 1988 AND 1992 RESPECTIVELY.

THE JAPANESE FUSIN PROGRAM IS RELATIVELY NEWER THAN THE OTHER THREE MAJOR PROGRAMS, BUT IS MOVING RAPIDLY TOWARD FULL PARITY. THE PROGRAM OF THE JAPANESE ATOMIC ENERGY RESEARCH INSTITUTE (JAERI), UNDER THE SCIENCE AND TECHNOLOGY AGENCY, IS CONCENTRATING ON THE TOKAMAK AND ON FUSION TECHNOLOGY. THE JT-60 TOKAMAK, WHICH WILL BEGIN OPERATION WITHIN ONE YEAR, WILL HAVE CONFINEMENT CAPABILITIES COMPARABLE TO THOSE OF TFTR ALTHOUGH JT-60 IS NOT DESIGNED FOR DEUTERIUM-TRITIUM OPERATION. CONCEPTUAL DESIGN STUDIES ARE IN PROGRESS FOR A NEW MAJOR TOKAMAK EXPERIMENT, THE FUSION EXPERIMENTAL REACTOR (FER), TO OPERATE IN THE MID TO LATE 1990'S. FER WOULD HAVE OBJECTIVES SIMILAR TO THOSE OF NET. THE FUSION TECHNOLOGY PROGRAM IS COMPARABLE TO THE U.S. PROGRAM ALTHOUGH NOT AS BROAD.

THE COMMITTEE DID NOT LOOK INTO THE FUSION PROGRAM OF THE SOVIET UNION. HOWEVER, IT IS KNOWN THAT THE USSR

PROGRAM IS ADVANCED TO A LEVEL COMPARABLE WITH THAT OF THE OTHER THREE MAJOR PROGRAMS. THE USSR PROGRAM HAS HISTORICALLY BEEN CHARACTERIZED BY STRONG SCIENTIFIC INSIGHT. PAST COOPERATION WITH THE USSR HAS BEEN TECHNICALLY FRUITFUL AND COULD BENEFICIALLY BE EXPANDED FROM THE RATHER MODEST CURRENT LEVELS IF U.S. POLICY CONSTRAINTS CHANGE. POLICY ISSUES COULD CHANGE SUFFICIENTLY IN THE FUTURE TO MAKE RENEWED SCIENTIFIC COOPERATION WITH THE USSR DESIRABLE; IN THAT EVENT, FUSION WOULD BE A SUITABLE VEHICLE.

CONCLUSIONS

A FORMAL BILATERAL AGREEMENT WITH JAPAN HAS COVERED MANY COOPERATIVE ACTIVITIES OVER THE PAST FEW YEARS. THERE EXIST FORMAL, MULTILATERAL AGREEMENTS AMONG THE U.S., JAPAN AND THE EC FOR SEVERAL COOPERATIVE ACTIVITIES UNDER THE AEGIS OF THE IEA. THE U.S., JAPAN, EC, AND USSR UNDER THE IAEA ARE COOPERATING IN THE INTERNATIONAL TOKAMAK REACTOR (INTOR). PREVIOUS COOPERATIVE UNDERTAKINGS IN FUSION HAVE BEEN SUBSTANTIAL AND GENERALLY SUCCESSFUL. WITH THIS BACKGROUND, THE COMMITTEE CONCLUDED:

PAST COOPERATION PROVIDES A SOUND BASIS FOR FUTURE EFFORTS

THE EXTENT TO WHICH ANY NATIONAL OR MULTINATIONAL FUSION PROGRAM WILL BE WILLING TO RELY ON INTERNATIONAL COOPERATION RATHER THAN ITS OWN STRENGTH AND DIRECTION IS A POLICY ISSUE; THE RESOLUTION OF WHICH MAY PLACE CONSTRAINTS UPON SUCH COOPERATION. THE MAIN INCENTIVES FOR INCREASED INTERNATIONAL COOPERATION ARE THE EXPECTATION OF ENHANCED TECHNICAL RESULTS, PROBABLY CUMULATIVE SAVINGS -- THROUGH SHARING OF COST AND RISK -- IN HUMAN AND FINANCIAL RESOURCES COMPARED TO THOSE REQUIRED BY A SEPARATE PROGRAM AND LONG-RUN MERIT AS SEEN AT THE HEADS-OF-STATE LEVEL. FOR THOSE AND OTHER REASONS THE COMMITTEE CONCLUDED:

ON BALANCE, THERE ARE SUBSTANTIAL, POTENTIAL BENEFITS OF LARGE-SCALE INTERNATIONAL COLLABORATION IN THE DEVELOPMENT OF FUSION ENERGY

THE POINTS MADE PREVIOUSLY CONCERNING THE APPROXIMATE PARITY IN THE STATUS OF THE WORLD PROGRAMS, THEIR SIMILARITY IN OBJECTIVES, THE GATHERING MOMENTUM OF THE EC AND JAPANESE PROGRAMS, THE EXISTENCE OF TECHNICAL NEEDS AND OPPORTUNITIES, POLITICAL AND ADMINISTRATIVE RECEPTIVITY, AND THE ABSENCE OF NEAR-TERM COMPETITION IN THE COMMERCIALIZATION OF FUSION SUPPORT THE FOLLOWING CONCLUSION:

A WINDOW IN TIME FOR LARGE-SCALE COLLABORATION IS NOW OPEN

THE EC AND JAPANESE FUSION PROGRAM PLANS HAVE BEEN DEVELOPED IN DETAIL FOR THE NEXT FEW YEARS AND RESOURCE COMMITMENTS HAVE BEEN MADE ACCORDINGLY. ANY MAJOR COLLABORATION MUST MEET THE REQUIREMENTS OF THE SEPARATE NATIONAL PROGRAMS AND THEREFORE MUST BE PRECEDED BY JOINT PLANNING. BROADER U.S. POLICY CONSIDERATIONS MAY BE AT ODDS WITH TECHNICAL OPPORTUNITIES FOR COOPERATION. THE USSR HAS PROPOSED JOINT INTERNATIONAL CONSTRUCTION OF THE NEXT STEP TOKAMAK EXPERIMENT, YET IT IS UNLIKELY THAT U.S. - USSR COLLABORATION IS POSSIBLE IN THE CURRENT CIRCUMSTANCES. JAPAN IS WILLING TO DISCUSS FURTHER MAJOR COLLABORATION, BUT

IN THE U.S. THERE EXISTS A POLITICAL SENSITIVITY TO JAPAN ON ECONOMIC GROUNDS. ON THE OTHER HAND, THE EC, WITH WHOM COLLABORATION WOULD BE THE LEAST CONTROVERSIAL, SHOWS LITTLE INTEREST. THESE POINTS ARE REALIZED TO THE FOLLOWING CONCLUSION:

> LARGE-SCALE INTERNATIONAL COLLABORATION CAN BE ACHIEVED BUT NOT QUICKLY

THE U.S. GOVERNMENT IS PERCEIVED BY SOME AS LACKING A FIRM COMMITMENT AND A REALISTIC PLAN TO DEVELOP FUSION. A CLEAR POLICY STATEMENT ON THE GOALS OF THE U.S. FUSION PROGRAM AND A CORRESPONDING PLAN TO MEET THESE GOALS NOT ONLY WOULD BE HELPFUL FOR EVALUATING PROPOSED MAJOR INTERNATIONAL COOPERATIVE PROJECTS BUT WOULD ALSO IMPROVE PERCEPTIONS OF THE U.S. COMMITMENT. THE U.S. IS ALSO PERCEIVED BY SOME AS AN "UNRELIABLE PARTNER." THE ANNUAL FUNDING APPROPRIATION PROCESS MAKES IT DIFFICULT FOR THE U.S. TO COMMIT TO MULTIYEAR PROJECTS WITHOUT THE POSSIBILITY OF FACING A CHOICE LATER OF EITHER GOING BACK ON THE COMMITMENT OR SACRIFICING OTHER ELEMENTS OF THE FUSION PROGRAM. REQUESTING EXPLICIT BUDGET ITEMS FOR INTERNATIONAL PROJECTS, AFTER CLEAR IDENTIFICATION OF THE OBLIGATIONS IMPLIED FOR SUBSEQUENT YEARS, MAY EASE THE PROBLEM. THE ABOVE FACTORS RESULT IN THE FOLLOWING CONCLUSION:

INTERNATIONAL COLLABORATION WILL REQUIRE STABLE INTERNATIONAL COMMITMENTS

TECHNOLOGY TRANSFER ARISES AS AN ISSUE AND A POSSIBLE CONSTRAINT IN THREE AREAS: NATIONAL SECURITY, PROTECTION OF U.S. INDUSTRY, AND LOSS OF ADVANTAGE TO FOREIGN PARTICIPANTS FROM TECHNOLOGY DEVELOPED BY THEM BECAUSE OF PROVISIONS OF THE U.S. FREEDOM OF INFORMATION ACT. HOWEVER, TECHNOLOGY TRANSFER DOES NOT SEEM TO BE A MAJOR CONCERN AT THIS TIME BECAUSE OF THE REMOTENESS OF SIGNIFICANT MILITARY OR COMMERCIAL APPLICATIONS OF MAGNETIC FUSION.

THERE ARE NUMEROUS INSTITUTIONAL CHOICES FOR IMPLEMENTATION OF INTERNATIONAL COOPERATIVE AGREEMENTS. TREATIES CONSTITUTE THE MOST BINDING COMMITMENTS TO THE U.S. GOVERNMENT BUT ARE THE MOST DIFFICULT AGREEMENTS TO CONCLUDE. EXISTING INTERNATIONAL ORGANIZATIONS SUCH AS IEA AND IAEA OFFER AUSPICES UNDER WHICH MORE EXTENSIVE INTERNATIONAL COOPERATION COULD BE CARRIED OUT WITHOUT THE NECESSITY OF NEW IMPLEMENTING AGREEMENTS. HOWEVER, NEITHER OF THESE AGENCIES OR OTHER EXISTING INTERNATIONAL ORGANIZATIONS WOULD BE SUITABLE AS SPONSORS FOR A MAJOR INTERNATIONAL PROJECT BECAUSE THEY FUNCTION PRIMARILY AS COORDINATORS AND ADMINISTRATORS AND NOT AS MANAGERS SINCE THEY HAVE THEIR OWN PRIORITIES. GENERALLY, A JOINT INTERNATIONAL PROJECT IS COMPLICATED, BUT IT CAN WORK IF IT

IS CAREFULLY PLANNED AND EXECUTED. THE COMMITTEE THEREFORE CONCLUDED:

> THERE ARE A HOST OF CONSIDERATIONS
> THAT MUST BE RESOLVED IN IMPLEMENTATION,
> BUT THESE APPEAR WORKABLE

IN THE COURSE OF ITS DOMESTIC WORKSHOPS AND ITS TWO OVERSEAS TRIPS, THE COMMITTEE COVERED A WIDE RANGE OF TOPICS CONCERNED WITH INTERNATIONAL COOPERATION IN THE DEVELOPMENT OF CONTROLLED MAGNETICALLY CONFINED FUSION. THE STUDY CONSIDERED "COOPERATION" IN THE GENERAL SENSE OF ACTING WITH OTHERS FOR MUTUAL BENEFIT ON EITHER A SMALL OR A LARGE SCALE AND "COLLABORATION" IN A SOMEWHAT MORE SPECIFIC SENSE OF WORKING ACTIVELY TOGETHER AS APPROXIMATELY EQUAL PARTNERS IN SIZEABLE ENTERPRISES.

THE VARIOUS MEETINGS IDENTIFIED THREE QUALITATIVELY DIFFERENT PATHS TO FUSION ENERGY THAT ARE OPEN TO THE UNITED STATES,

1) TO MAKE THE COMMITMENT TO BECOME THE ALL-OUT COMPETITIVE LEADER IN ALL ITS ASPECTS

2) TO ENGAGE IN LARGE-SCALE INTERNATIONAL COLLABORATION

3) To withdraw with the intent of purchasing the developed technology from others in the future.

Reviewing the above and the individual conclusions stated earlier, the overall conclusion derived by the committee was:

For the U.S. at this time, large-scale international collaboration is preferable to a mainly domestic program which would have to command substantial additional resources for the competitive pursuit of fusion energy development or run the risk of forfeiture of equality with other world programs.

RECOMMENDATIONS

GIVEN THIS OVERALL CONCLUSION, TWO MAJOR RECOMMENDATIONS FOLLOW:

> THE FIRST PRIORITY SHOULD BE THE ESTABLISHMENT OF A CLEAR SET OF POLICIES AND OBJECTIVES AND A CONSIDERED PROGRAM PLAN FOR FUTURE U.S. FUSION ACTIVITIES.

THE ABOVE IS A NECESSARY PREREQUISITE FOR DISCUSSION WITH POTENTIAL PARTNERS AND FOR ANY LONG-RANGE COMMITMENTS THAT ENSUE.

> HAVING CARRIED OUT THE PRECEDING RECOMMENDATION, THE U.S. SHOULD TAKE THE LEAD IN CONSULTING WITH PROSPECTIVE PARTNERS TO INITIATE A JOINT PLANNING EFFORT AIMED AT LARGE-SCALE COLLABORATION.

THIS JOINT PLANNING ACTIVITY WOULD HAVE TO INVOLVE GROUPS AT THE PROGRAM LEADERSHIP LEVEL AND AT THE TECHNICAL LEADERSHIP LEVEL IN APPROPRIATE ROLES AND WOULD HAVE TO BE A CONTINUING FOCUSED ACTIVITY OVER MANY YEARS.

Mrs. LLOYD. Thank you very much not only for your insight, but for your very excellent statement. Looking over the report from the Fusion Advisory Committee, under implications for cooperation the statement was made that the programs in the Soviet Union, Japan, and the European Community were at comparable stages of development with the exception of the slippage of TFCX. Do you feel that the difference is still more in perception or is it in actual advantage over other countries?

Mr. GAVIN. I'm not sure what the half life of our position is, but surely a year ago we thought things were rather comparable. I'll have to defer to one of my more expert colleagues with regard to whether that condition exists today.

Mrs. LLOYD. Turning to international collaboration, how successful has this been in West Germany in the area of ash and impurity removal? How does this work relate to the R&D work at Princeton?

Mr. GAVIN. My understanding is that we have people involved in these undertakings. I think that in the less than major experimental devices, there have been many examples of excellent collaboration and there continue to be. One of the things that was pretty obvious is that the fusion community as a whole has worked rather well together over the years. I think that is certainly something that can be expected to continue. I think the plight addressed by the committee was substantially who can afford to build the next European torus or who can afford to build the FER in Japan or what was called the next step here in this country. Would it not make sense to collaborate on them. I think that it does make sense, but the point of the report simply is that you don't do it by wishing it. It's going to take a lot of detailed expert work to establish a basis for collaboration because the investment's being made whether here or in Europe or in Japan are going to be examined very carefully to see whether the investments are producing worthwhile results and whether the shares involved are perceived to be fair shares.

Mrs. LLOYD. Do you think that DOE has the will and the firmness to come up with a real policy and a program to lead for international cooperation? Is the resolve there in the Department?

Mr. GAVIN. I'm not sure, quite frankly. I personally believe that this is an area where the administration should take the lead and set a policy that says that we are going to pursue the development of fusion power according to some time schedule. I'm not saying that we are going to land on the moon in 10 years, but when Mr. Kennedy established that goal, people did pay attention to it and things happened. I think that a President carries a lot of weight when he signals out a particular line of endeavor and says that this has a priority attached to a time schedule.

Mrs. LLOYD. What I was basically referring to is a commitment that's more than lipservice. An example of this is the week before the vote on Clinch River came up before our full committee. I was visiting the White House because the President was very interested in my vote on the budget. I said that I was also very concerned about Clinch River. He said that he was committed to Clinch River. He turned to one of the members of his Cabinet and they said yes, they were also very committed and that was the last of it. There was no commitment and no determination to really show their resolve in the Congress to support the program more than lipservice. The point that I'm trying to make is it's going to take more than lipservice. It's going to take the resolve of saying that we will do this and these are the steps we're taking to move for international cooperation.

Mr. GAVIN. I'll agree with you, Madam Chairman. It will take a great deal more than lipservice and I think it does require a very long-term view. If you look at our situation with the budget today, many of the problems we have represent the problem of compromising the short-term view with the long-term goals. Surely this is one of the longer term goals that we have in our scan at the minute. I think it's very difficult to say how to balance this against some commitment that will pay off in 2 years. This is a long-term goal and it requires an understanding that the demands for energy are going to increase and that the sources of energy are unlikely to become less expensive as time goes by, or more available.

It does take a long-term commitment and it does take a very bold position.

Mrs. LLOYD. In my visits to other countries, it causes me a great deal of pain when I visit our meetings on energy matters to realize that we are considered an unreliable partner. This is very painful for me. I was wondering what the Japanese reaction was to the termination of the FMIT activities in this country.

Mr. GAVIN. I think—I'm not sure I can contribute anything new to that other than to report that as I heard it, the Japanese position was simply that we have our program; we are going to make it go; we are not going to jeopardize it by getting committed irrevocably to anybody else's program; if along the way we can do something that is mutually useful, we will be interested to examine what can be done. When we have a priority that's been established and we intend to carry it forward. The motivation there is much more clear cut. It has the characteristic of having been decided upon.

Mrs. LLOYD. Thank you very much, Mr. Gavin, for your fine testimony. Mr. Fawell do you have any questions? We thank you for being with us today.

Our next witness is Dr. Ron Davidson from the DOE Magnetic Fusion Advisory Committee.

We welcome you, Dr. Davidson, as chairman of the committee and look forward to your testimony; you may summarize or proceed as you wish.

STATEMENT OF DR. RONALD C. DAVIDSON, CHAIRMAN, MAGNETIC FUSION ADVISORY COMMITTEE AND DIRECTOR, MIT PLASMA FUSION CENTER

Dr. DAVIDSON. Madam Chairman, I am pleased to have this opportunity to address this subcommittee on behalf of the Magnetic Fusion Advisory Committee, which was formed in June 1982. As you know, the Magnetic Fusion Advisory Committee [MFAC], provides advice and guidance on a continuing basis to the Secretary of Energy through the Director of Energy Research on the Magnetic Fusion Energy Program.

The present membership of MFAC is listed on the first viewgraph. There is broad institutional representation on this committee from industry, universities, and the national laboratories. Also the committee membership has strong technical expertise in a wide range of engineering and scientific areas. It is a pleasure to serve with such a distinguished and dedicated group.

The second and third viewgraphs give an outline and chronology of charge areas in which we have provided advice to the Department of Energy at the request of Dr. Alvin W. Trivelpiece. As you can see, we have had a very busy schedule, and the charges to MFAC have covered virtually every facet of the Magnetic Fusion Energy Program.

I have reported to you on three previous occasions our recommendations to DOE in several key program areas. Today, I plan to outline our activities covering the period since my testimony to this subcommittee in February 1984.

The program areas to be covered are:

First, the role of industry in the fusion program.

Second, evaluation of supporting fusion concepts.

Third, evaluation of options for an ignition experiment.

Fourth, evaluation of the draft DOE fusion program plan.

My oral remarks in each area will be relatively brief. However, with your permission, a more detailed account of MFAC findings and recommendations will be submitted for the record.

First, the Magnetic Fusion Advisory Committee commends the many significant technical contributions of industry to the fusion program over the years, and urges that mechanisms be found to maintain strong industrial expertise and participation in the program in the future. In times of budgetary stringency, this may require contractual mechanisms to ensure stable relationships between the program and those industries with a strong commitment to fusion development.

Second, there should be a continued effort to broaden the intellectual involvement of industry in task areas where industrial experience can benefit the fusion program. Assignments in such task areas will permit the full scope of industrial skills to be developed and utilized.

Finally, industry needs a clearer statement of fusion program goals and approximate timing from DOE in order to properly assess corporate priorities and commitments to this important effort.

Here, supporting concepts refer to the Elmo Bumpy Torus, Field Reversed Configurations, Reversed Field Pinches, Spheromaks, and Stellarators.

It is our assessment that the supporting concepts activities have made large contributions to the physics, technology and reactor conceptualization of the fusion program. These research activities are making impressive technical progress in their own right, and they also contribute to the tokamak and mirror programs through advances in the basic understanding of plasma confinement properties, and through the development of advanced technologies. We recommend continued strong support for the supporting concepts activities.

In the constrained budget circumstances of July 1984, MFAC concluded that the level of support for the supporting concepts activities as a group was about correct. However, some redistribution of funding along the lines of the priorities established by MFAC Panel No. 9 would be appropriate. Specifically, the Field Reversed Configurations and the Reversed Field Pinch Programs were recommended by MFAC Panel No. 9 for increases, the recommended EBT program represented a decrease over earlier levels, and the recommended Spheromak Program would lead to reduced budgets over the next few years.

We recommend that continued strong support be given to the ATF Base Program at Oak Ridge National Laboratory, and to supporting Stellarator activities. As presently designed, the ATF experiment will provide a significant complement to foreign Stellarator experiments, and make strong contributions to toroidal concept development.

Finally, it is our assessment that progress in research on the Reversed Field Pinch at Los Alamos National Laboratory and at GA Technologies has been outstanding, and that this concept is technically ready to proceed device that has toroidal current capability in the two megampere range or beyond.

In July 1984, the Magnetic Fusion Advisory Committee reviewed the results of the national TFCS preconceptual design study, headed by the Princeton Plas Physics Laboratory—PPPL. We found the objectivity and coherence of this multi-institutional design effort to be commendable.

Consistent with the stringent budgetary circumstances, let me summarize our principal findings and recommendations in this important area.

First, the Magnetic Fusion Advisory Committee believes that the experimental study of ignition and equilibrium burn is the next major frontier of magnetic fusion research.

We also recommend continued study of innovative options for achieving the burning-plasma state. Specifically we recommend development of two options with distinctly different objectives: (1) a copper-coil design that is optimized for minimum cost, while meeting the basic scientific objectives; (2) a superconducting-coil design that is optimized for the highest possible scientific and technological relevance and upgradability.

The copper-coil design should be oriented toward innovative techniques in plasma handling and magnetics, should introduce appropriately controlled adjustments in the present set of standard design specifications, and should exploit the existing hardware assets of the National Magnetic Fusion Program to the maximum possible extent. The planning and scheduling for this option should emphasize consistency with realistic near-term funding expectations and with continued near-term productivity of the U.S. Toroidal Confinement Program.

The superconducting-coil design option should aim at the fullest possible development of both the science and technology of burning plasmas. Planning and scheduling should be realistically in line with the likely magnitude of the required project funding, and the required data bases in plasma physics, materials science, and magnetics.

We believe that the two types of options for an ignition experiment should be considered complementary rather than strictly competitive. As world fusion research evolves into this burning-plasma phase, optimized facilities of both the recommended types seem likely to become major parts of a cooperative international fusion effort.

At its meeting last month, the Magnetic Fusion Advisory Committee completed its review of the draft DOE Magnetic Fusion Program Plan—February 1985. This plan has not been formally transmitted to Congress by DOE, and I will limit my remarks to a characterization of our findings and recommendations.

It is our assessment that the February 1985 draft of the plan is clearly reasoned, well-articulated and effectively organized. It should constitute a very useful policy framework for the Magnetic Fusion Program.

Let me quote the motion passed unanimously by MFAC on this subject at its meeting on February 25:

> The Magnetic Fusion Advisory Committee fully endorses and supports the February, 1985 U.S. Department of Energy Magnetic Fusion Program Plan as a strategic framework for carrying out the program in the present national circumstances. Broad and rapid dissemination of this Program Plan is recommended. Fusion is recognized as a potentially abundant and environmentally attractive energy source, which could sustain industrial society in the 21st Century and beyond. The Committee emphasizes that the period 1985-90 is foreseen as a period of intense scientific and technological progress, with TFTR providing experience with plasmas at near-reactor conditions, and technical advances expected in tokamaks, tandem mirrors, stellarators, and reversed field pinches.

That concludes my remarks on behalf of the Magnetic Fusion Advisory Committee. Thank you for your continued strong support of the Magnetic Fusion Program.

[The prepared statement of Dr. Davidson follows:]

Madam Chairman, I am pleased to have this opportunity to address this Subcommittee on behalf of the Magnetic Fusion Advisory Committee, which was formed in June, 1982. As you know, the Magnetic Fusion Advisory Committee (MFAC) provides advice and guidance on a continuing basis to the Secretary of Energy through the Director of Energy Research on the Magnetic Fusion Energy Program.

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I have reported to you on three previous occasions our recommendations to DOE in several key program areas. Today, I plan to outline our activities covering the period since my testimony to this Subcommittee in February, 1984.

The program areas to be covered are:

1. The role of Industry in the fusion program (Charge Area #7).
2. Evaluation of supporting fusion concepts (Charge Area #9).
3. Evaluation of options for an ignition experiment (Charge Area #11).
4. Evaluation of the draft DOE fusion program plan (Charge Area #12).

My oral remarks in each area will be relatively brief. However, with your permission, a more detailed account of MFAC findings and recommendations will be submitted for the record.

Role of Industry in the Fusion Program

First, the Magnetic Fusion Advisory Committee commends the many significant technical contributions of industry to the fusion program over the years, and urges that mechanisms be found to maintain strong industrial expertise and participation in the program in the future. In times of budgetary stringency, this may require contractual mechanisms to ensure stable relationships between the program and those industries with a strong commitment to fusion development.

Second, there should be a continued effort to broaden the intellectual involvement of industry in task areas where industrial experience can benefit the fusion program. Assignments in such task areas will permit the full scope of industrial skills to be developed and utilized.

Finally, as recommended by MFAC Panel #7, industry needs a clearer statement of fusion program goals and approximate timing from DOE in order to properly assess corporate priorities and commitments to this important effort.

Supporting Fusion Concepts

Here, supporting concepts refer to the Elmo Bumpy Torus, Field Reversed Configurations, Reversed Field Pinches, Spheromaks, and Stellarators.

It is our assessment that the supporting concepts activities have made large contributions to the physics, technology and reactor conceptualization of the fusion program. These research activities are making impressive technical progress in their own right, and they also contribute to the tokamak and mirror programs through advances in the basic understanding of plasma confinement properties, and through the development of advanced technologies. We recommend continued strong support for the supporting concepts activities.

In the constrained budget circumstances of July, 1984, MFAC concluded that the level of support for the supporting concepts activities as a group was about correct. However, some redistribution of funding along the lines of the priorities established by MFAC Panel #9 would be appropriate. Specifically, (a) the Field Reversed Configurations and the Reversed Field Pinch programs were recommended by MFAC Panel #9 for increases, (b) the recommended EBT program represented a decrease over earlier levels, and (c) the recommended Spheromak program would lead to reduced budgets over the next few years.

We recommend that continued strong support be given to the Advanced Toroidal Facility (ATF) base program at Oak Ridge National Laboratory, and to supporting stellarator activities. As presently designed, the ATF experiment will provide a significant complement to foreign stellarator experiments, and make strong contributions to toroidal concept development.

Finally, it is our assessment that progress in research on the Reversed Field Pinch at Los Alamos National Laboratory and at GA Technologies has been outstanding, and that this concept is technically ready to proceed with a device that has toroidal current capability in the 2 megampere range or beyond.

Options for an Ignition Experiment

In July, 1984, the Magnetic Fusion Advisory Committee reviewed the results of the national TFCX preconceptual design study, headed by the Princeton Plasma Physics Laboratory (PPPL). We found the objectivity and coherence of this multi-institutional design effort to be commendable.

Consistent with the stringent budgetary circumstances, let me summarize our principal findings and recommendations in this important area.

First, the Magnetic Fusion Advisory Committee believes that the experimental study of ignition and equilibrium burn is the next major frontier of magnetic fusion research.

We also recommend continued study of innovative options for achieving the burning-plasma state. Specifically, we recommend development of two options with distinctly different objectives: (1) a copper-coil design that is optimized for minimum cost, while meeting the basic scientific objectives; (2) a superconducting-coil design that is optimized for the highest possible scientific and technological relevance and upgradability.

The copper-coil design should be oriented towards innovative techniques in plasma-handling and magnetics, should introduce appropriately controlled adjustments in the present set of standard design specifications, (for example, reduced burn time), and should exploit the existing hardware assets of the national magnetic fusion program to the maximum possible extent. The planning and scheduling for this option should emphasize consistency with realistic near-term funding expectations and with continued near-term productivity of the U.S. toroidal confinement program.

The superconducting-coil design option should aim at the fullest possible development of both the science and technology of burning plasmas. Planning and scheduling should be realistically in line with the likely magnitude of the required project funding and the required data bases in plasma physics, materials science, and magnetics.

We believe that the two types of options for an ignition experiment should be considered complementary rather than strictly competitive. As world fusion research evolves into its burning-plasma phase, optimized facilities of both the recommended types seem likely to become major parts of a cooperative international fusion effort.

Draft DOE Fusion Program Plan

At its meeting last month, the Magnetic Fusion Advisory Committee completed its review of the draft DOE Magnetic Fusion Program Plan (February, 1985). This plan has not been formally transmitted to Congress by DOE, and I will limit my remarks to a characterization of our findings and recommendations.

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That concludes my remarks on behalf of the Magnetic Fusion Advisory Committee. I would now like to offer some personal observations and comments.

It is very important that high priority be given to developing implementation plans for the general strategy outlined in the DOE Magnetic Fusion Program Plan. The elements of the implementation plans should emphasize a strong program in fusion physics and technology with the following principal features.

First, there should be a vigorous base research program, emphasizing both increased understanding and improved concepts (advanced tokamaks, tandem mirror, and alternate concepts). The thrust of concept improvement activities should be both to increase scientific understanding and to improve reactor prospects. Historically, the interplay between these two lines of effort has led to our most creative physical insights and concepts. Such a program is essential to technical progress and to the training of talented new people.

Second, the demonstration and experimental study of an ignited fusion plasma is the obvious next research frontier after attainment of scientfic breakeven. While the scientific understanding of many key plasma phenomena can best be gained on moderate-size experimental facilities, ultimately plasma confinement properties must be investigated under conditions of intense alpha-particle heating, which will require an ignited plasma core. The fusion program is at the point where consideration of such experiments can proceed with a substantial degree of realism. Specific design ideas will continue to evolve rapidly as results from experiments, particularly TFTR and JET, become available in the next several years. In the near-term, the design studies for an ignition experiment should emphasize maximum scientific output with minimum project cost.

That concludes my oral testimony. Thank you for your continued strong support of the magnetic fusion program.

MAGNETIC FUSION ADVISORY COMMITTEE

CHARTER, MEMBERSHIP AND CHRONOLOGY OF CHARGE AREAS

MAGNETIC FUSION ADVISORY COMMITTEE

CHARTERED AREAS OF ADVICE AND GUIDANCE TO DOE

- PERIODIC REVIEWS OF ELEMENTS OF THE PROGRAM AND RECOMMENDATION OF ANY CHANGES CONSIDERED DESIRABLE ON THE BASIS OF SCIENTIFIC AND TECHNOLOGICAL ADVANCES OR OTHER FACTORS

- ADVICE ON LONG-RANGE PLANS, PRIORITIES, AND STRATEGIES TO DEMONSTRATE THE SCIENTIFIC AND ENGINEERING FEASIBILITY OF FUSION

- ADVICE ON RECOMMENDED APPROPRIATE LEVELS OF FUNDING TO DEVELOP THOSE STRATEGIES AND TO HELP MAINTAIN APPROPRIATE BALANCE BETWEEN COMPETING ELEMENTS OF THE PROGRAM

- ADVICE AND GUIDANCE ON ANY ISSUES RELATING TO THE PROGRAM AS REQUESTED BY THE SECRETARY OR THE DIRECTOR OF ENERGY RESEARCH

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Marshal N. Rosenbluth
University of Texas

Alan Schriesheim
Argonne National Laboratory

Weston M. Stacey, Jr.
Georgia Institute of Technology

	AREA OF MFAC EVALUATION	PANEL CHAIRPERSON	DOE CHARGE RECEIVED	MFAC RECOMMENDATIONS TO DOE
#1	Tandem Mirrors & Tokamaks	Dr. John Gilleland G.A. Technologies	June, 1982	November, 1982
#2	Alternate Confinement Concepts	Prof. Robert Conn UCLA	June, 1982	November, 1982
#3	TFTR Upgrade Options	Dr. Harold Forsen Bechtel	June, 1982	January, 1983
#4	MFTF-B Upgrade Options	Dr. John Gilleland G.A. Technologies	January, 1982	May, 1983
#5	Long-Term University Role	Prof. Robert Gross Columbia University	January, 1983	August, 1983
#6	Program Priorities	Prof. Ronald Davidson MIT	May, 1983	September, 1983
#7	Role of Industry	Dr. Harold Forsen Bechtel	September, 1983	May, 1984

	Area of MFAC Evaluation	Panel Chairperson	DOE Charge Received	MFAC Recommendations to DOE
#8	Ignition Experiment Priority	Full Committee	February, 1984	February, 1984
#9	Supporting Concepts	Dr. Lee Berry ORNL	February, 1984	July, 1984
#10	High Power Density Systems	Prof. Robert Conn UCLA	May, 1984	Pending
#11	Options for Ignition Experiment	Full Committee	July, 1984	July, 1984
#12	DOE Draft Program Plan	Prof. Ronald Davidson MIT	October, 1984	February, 1985
#13	Fusion Systems Studies	Prof. Weston Stacey, Jr. Georgia Tech.	February, 1985	Pending

MAGNETIC FUSION ADVISORY COMMITTEE

RECOMMENDATIONS TO THE DEPARTMENT OF ENERGY

Department of Energy
Washington, D.C. 20585

September 23, 1983

Dr. Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee
Director, Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street, NW16-202
Cambridge, MA 02139

Dear Ron:

To assist the Department in making effective use of private sector participation in fusion energy development, I would like the Magnetic Fusion Advisory Committee (MFAC) to advise us on specific issues related to the role of industry in the fusion program.

Considerable attention has been addressed to this issue in the Comprehensive Program Management Plan (CPMP), the National Research Council (NRC) report on Future Engineering Needs of Magnetic Fusion, and discussions at ERAB and MFAC meetings. The CPMP indicates the Department's near-term intent to maintain the existing "substantial industrial participation", while the NRC report recommends long-term planning to assure "...a framework can be established for the transition from Federal to private financial support in the next century."

We would like more specific advice on the industrial role that would be valuable in achieving current near-term fusion program objectives and in preparing for the achievement of long-term fusion program objectives.

Industry has been and will continue to be involved in engineering design and in supplying the components needed for fusion development. However, the future role recommended by the NRC study requires systematically increasing the intellectual involvement of industry. As the NRC report further noted, this must be done with full recognition of the timescale involved in fusion development. In the past, the program has attempted to achieve this through various mechanisms. The establishment of the Fusion Engineering Design Center, the MFTF-B cost sharing development effort, the procedure established in the EBT-P program, and recently in the suggestion for industrial participation in the TFCX design process. We would like your evaluation of the benefits and liabilities inherent in these efforts as well as suggestions for other mechanisms to use in the future.

In addition, would you address the following issues: Recognizing that industry is not monolithic, can there be a general industrial role in fusion? In particular, to what extent should industry have responsibility for entire component systems? At what stage should industry have full responsibility for the operation of fusion devices? Is there an appropriate role for industry in the operation of experiments at our national laboratories? What role should industry play in the development of fusion technology? What role should cost-sharing with industry play in the fusion program. What are the reasons for involvement by various types of industries, and how can a synergistic program be structured to satisfy the separate objectives of industry and government.

In your final report, please include recommendations on mechanisms for industrial participation and priorities for types of work in two separate programs: a constant, level-of-effort base program and a separate reactor engineering program.

I would like your report by June 1984.

Sincerely,

AWTrivelpiece

Alvin W. Trivelpiece
Director, Office of
Energy Research

May 3, 1984

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
Department of Energy
Washington, D.C. 20545

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee (MFAC) met in Oak Ridge, Tennessee, on May 1 and 2, 1984, and heard presentations by MFAC Panel #7 on the role of industry in the fusion program. We endorse the principal findings and recommendations (attached) of the panel, and the final report is enclosed for your perusal. In addition, the Magnetic Fusion Advisory Committee wishes to make the following ancillary observations and recommendations at this time:

1. MFAC takes note of the many significant technical contributions of industry to the fusion program over the years, and urges that mechanisms be found to maintain strong industrial expertise and participation in the program in the future.
2. In times of budgetary stringency, this may require contractual mechanisms to insure stable relationships between the program and those industries with a strong commitment to fusion development.
3. There should be a continued effort to broaden the intellectual involvement of industry in task areas where industrial experience can benefit the fusion program. Assignments in such task areas will permit the full scope of industrial skills to be developed and utilized.
4. The DOE should work towards a uniform policy within the international fusion community with regard to extranational procurements.

5. The panel has evaluated several models for industrial involvement in major fusion projects (TFTR, MFTF-B, EBT-P). Each of these approaches has special merits and should be evaluated as candidate mechanisms for TFCX.
6. As the next major step in the fusion program, the TFCX project should be viewed as an opportunity to develop even broader mechanisms for industrial participation. MFAC believes that the approach that will be used during the initial design phase (Phase I) has merit. For subsequent phases of the TFCX, we strongly recommend that DOE explore meaningful industrial roles that can lead to longer-term commitments and interactive projects that will serve the national program.
7. Industry needs a clearer statement of fusion program goals and approximate timing from DOE in order to properly assess corporate priorities and commitments to this important effort.

We commend the panel for an excellent report and look forward to discussing these recommendations with you.

As a final point discussed at our May 2 meeting, we believe that engineering research is basic to fusion energy development. As fusion moves into its immediate next phase, an integrated engineering program should be maintained as a complement to other program elements. The acquisition of engineering data will be a key ingredient in future decisions on fusion facilities and technical program directions.

Sincerely,

Ron Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee

RCD:gnc
Enclosure

Department of Energy
Washington, D.C. 20585

FEB 06 1984

Dr. Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee
Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street, NW16-202
Cambridge, MA 02139

Dear Ron,

The Stellarator, Compact Toroid, Elmo Bumpy Torus, and Reversed Field Pinch concepts have all made significant contributions to our scientific understanding in this past year. It is apparent that these supporting concepts offer a prime opportunity to improve the scientific productivity of the fusion program. It appears that relatively small investments in this area can lead to rapid advances in our overall scientific understanding.

Several of these concepts are on the verge of substantial, technical advances; however, the realization of these advances will require a redistribution of program resources that depends on a relative value judgment. I would like the Committee's recommendation as to how the supporting concepts can be managed so as to maximize the production of scientific information for a given investment.

I am not requesting another detailed review of each of the supporting concepts. Instead, please identify where increased emphasis for specific concepts would result in better understanding of fusion physics and provide insight for the benefit of the overall fusion program.

A timely response to this question would be useful during the formulation of its forthcoming budgets. Accordingly, I ask that you provide me with the Committee's views by September 1984.

Sincerely,

Alvin W. Trivelpiece
Director, Office of
Energy Research

July 20, 1984

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee met in Princeton on July 18-20 and reviewed the findings and recommendations of the MFAC Panel evaluating the supporting concept activities (Panel No. 9). The concepts evaluated were the Elmo Bumpy Torus, Field Reversed Configurations, Reversed Field Pinches, Spheromaks, and Stellarators. The Magnetic Fusion Advisory Committee endorses in principle the findings and recommendations of the Panel (see attached Executive Summary of the Panel Report), and makes the following additional comments and recommendations:

1. We agree with the Panel that the supporting concepts activities have made "large contributions to the physics, technology and reactor conceptualization of the fusion program." These research activities are making impressive technical progress in their own right, and they also contribute to the tokamak and mirror programs through advances in the basic understanding of plasma confinement properties, and through the development of advanced technologies. We recommend continued strong support for the supporting concepts activities.

2. In the current constrained budget circumstances, the present level of support for the supporting concepts activities as a group is about correct. However, some redistribution of funding along the lines of the priorities established by the Panel would be appropriate. Specifically, (a) the FRC, Stellarator and RFP programs were recommended by the Panel for increases, (b) the recommended EBT program represented a decrease over FY 1984 levels, and (c) the recommended Spheromak program would lead to reduced bugdets over the next few years.

With regard to the two largest supporting concept programs, the following points are also made:

3. We agree with the Panel that continued strong support be given to the ATF base program and supporting stellarator activities. As presently designed, the ATF experiment will provide a significant complement to foreign stellarator experiments, and make strong contributions to toroidal concept development.

4. We agree with the Panel that progress in research on the Reversed Field Pinch has been outstanding, and that this concept is technically ready to proceed with a device that has toroidal current capability in the 2-MA range or beyond. The Committee recommends that present Los Alamos design options be examined to assure that the follow-on experiment to ZT-40 effectively complements the RFX experiment under construction at Padova and the proposed upgrade of OHTE at GA Technologies, Inc.

If there are any questions regarding these recommendations, please feel free to contact me.

Sincerely,

Ronald C Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory
Committee

RCD:dvm

Department of Energy
Washington, D.C. 20585

APR 4 1984

Dr. Ronald C. Davidson
Director, Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street
Room 200, NW21-204
Cambridge, MA 02139

Dear Ron,

In its February 10, 1984, letter, the Magnetic Fusion Advisory Committee (MFAC) recommended that the Tokamak Fusion Core Experiment (TFCX), a moderate-cost tokamak reactor device designed to achieve ignition and long-pulse equilibrium burn, be given top priority in the U.S. fusion program. In addition to other considerations, it was recommended that MFAC be consulted during the formulative process.

The Department of Energy (DOE) does intend to consult MFAC regarding the major aspects of TFCX. A key milestone that is scheduled for July 1984 is the selection of a reference design configuration(s) to use as the basis for conceptual design in FY85. In preparation for this selection, PPPL, the FEDC, and other laboratory participants are now in the process of evaluating various TFCX options. DOE would like MFAC to review the basic design process that is being used both to perform tradeoff studies and formulate design options. In particular, we are requesting the opinion of MFAC on four issues. Have all major, viable options for achieving ignition and long-pulse equilibrium burn been examined? Have all important design tradeoffs been considered? Have the tradeoffs been evaluated properly? Finally, what are the programmatic implications, both positive and negative, of the principle options being pursued by the TFCX design team? DOE needs your advice on this matter by the end of July 1984 to aid in our selection of a reference concept for further engineering analysis.

Since this matter is of high priority to the program, it would be desirable for the full committee to hear an interim TFCX presentation and discuss the matter at its May 1 and 2, 1984, meeting and then devote essentially full time to it at a special meeting on July 19 and 20, 1984. DOE would like to obtain your advice immediately thereafter.

I look forward to hearing from MFAC on this important matter.

Sincerely,

Alvin W. Trivelpiece
Director, Office of
Energy Research

July 20, 1984

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee met in Princeton on July 18-20 and reviewed the results of the national TFCX preconceptual design study, headed by the Princeton Plasma Physics Laboratory (PPPL). As a general remark, the Committee commends the objectivity and coherence of this multi-institutional design effort using common physics, engineering and costing assumptions. The design effort to date has established technically meaningful trade studies for a set of four basic TFCX options, which are labeled in terms of their characteristic toroidal-field systems: Nominal Superconducting, High-Performance Superconducting, Nominal Copper, and High-Performance Copper.

The TFCX design study has been guided by the recommendations of a national Technical Advisory Committee (TAC). We note the view of the TAC that, while the design studies already carried out have provided good coverage of the range of relevant options within the initially defined design constraints, there remain other important possibilities to be studied: for example, some that might lead to a lower-cost facility, or others with greater long-term potential and particular relevance to the possibility of major international collaboration.

The Magnetic Fusion Advisory Committee agrees with the TAC that the investigation of new design options is appropriate before a specific TFCX project is proposed. This approach is consistent both with the present budgetary climate and the desire of DOE that the program should pursue international collaboration.

With these points in mind, we make the following recommendations:

1. The Committee believes that the experimental study of ignition and equilibrium burn is the next major frontier of magnetic fusion research.

2. The Committee recommends continued study, during the next year, of innovative options for achieving the burning-plasma state. Strong design innovation groups should be established within the existing system of central organization and multi-institutional participation, to develop particular designs.

3. Specifically, the Committee recommends development of two options with distinctly different objectives: (1) a copper-coil design that is optimized for minimum cost, while meeting the basic scientific objectives; (2) a superconducting-coil design that is optimized for the highest possible scientific and technological relevance and upgradability. Neither of the two recommended design options corresponds exactly to any of the four options studied thus far.

4. The copper-coil design should be oriented towards innovative techniques in plasma-handling and magnetics, should introduce appropriately controlled adjustments in the present set of standard design specifications, (for example, reduced burn time), and should exploit the existing hardware assets of the national magnetic fusion program to the maximum possible extent. The planning and scheduling for this option should emphasize consistency with realistic near-term funding expectations and with continued near-term productivity of the U.S. toroidal confinement program.

5. The superconducting-coil design option should aim at the fullest possible development of both the science and technology of burning plasmas. Planning and scheduling should be realistically in line with the likely magnitude of the required project funding and the required data bases in plasma physics, materials science, and magnetics.

The Committee believes that the two specifically recommended options should be considered complementary rather than strictly competitive. As world fusion research evolves into its burning-plasma phase, optimized facilities of both the recommended types seem likely to become major parts of a cooperative international fusion effort.

We also note that the further evolution of the design studies will be strongly influenced by results and techniques being developed on major existing experimental facilities, such as TFTR and JET. Conversely, the overall planning of the next-generation ignition experiment should include specific recommendations for the optimization of the U.S. toroidal confinement program, particularly TFTR experimental operations.

If there are any questions regarding these recommendations, please feel free to contact me.

Sincerely,

Ronald C Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory
Committee

RCD:dvm

Department of Energy
Washington, D.C. 20585

PLASMA FUSION CENTER
OCT - 1984
RECEIVED

OCT 3 1984

Dr. Ronald C. Davidson
Plasma Fusion Center
Massachusetts Institute of Technology
167 Albany Street, NW16-202
Cambridge, MA 02139

Dear Dr. Davidson: – Ron

The fiscal year 1985 President's budget included $483M for the magnetic fusion program. In early July, Congress reduced this by $43M to $440M. It was necessary to distribute this reduction over the program elements before developing the fiscal year 1986 budget, which had to undergo internal departmental review in August.

The $43M reduction was taken in three principal activities. The Tokamak Fusion Test Reactor (TFTR) was reduced by $9M with the understanding that the D-T burning experiments would be delayed as a consequence. The Mirror Fusion Test Facility (MFTF-B) was reduced by $15M with the recognition that this would delay some of its goals by a year or more. The development and technology program was reduced by $13M with the understanding that the program would concentrate on the technologies required to support the main program activities and delay some of the reactor specific efforts. The $6M balance of the $43M reduction was taken over other program elements in smaller amounts.

Of necessity, these reductions have resulted in major changes in certain aspects of the fusion program strategy. These changes are reflected in the draft "Magnetic Fusion Policy Plan" (MFPP), prepared under John Clarke's guidance by the Office of Fusion Energy staff. At its October 4 meeting, I want MFAC to review the elements of this plan and prepare a report outlining any views, recommendations, or advice they may have on it. At this same meeting, I would like you to initiate a longer-term study of ways to implement this strategy.

The development and acceptance of a revised strategy for the fusion program is the beginning of a process that will have important consequences on the way in which this program develops in the United States. In view of the general thrust to encourage international collaboration in areas such as fusion, I suggest that you solicit views on the strategy and its implementation by some of the leaders of fusion programs outside the United States.

I have not established a firm date for either of the requested reviews, but obviously this is an important task that you undertake, and I would appreciate your efforts to submit a report as soon as reasonably possible.

Sincerely,

Alvin W. Trivelpiece
Director, Office of
Energy Research

Enclosure:
Draft Magnetic Fusion Policy Plan

cc:
John Clarke

February 25, 1985

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
U.S. Department of Energy
Washington, DC 20585

Dear Dr. Trivelpiece:

The Magnetic Fusion Advisory Committee met in Washington, DC on February 25, 1985 and completed its review of the draft DOE Fusion Program Plan (February, 1985). Overall, it is our assessment that the plan is clearly reasoned, well articulated, and effectively organized. It should constitute a very useful policy framework for the magnetic fusion program.

At the February 25 meeting, the following motion was approved unanimously by MFAC:

> "The Magnetic Fusion Advisory Committee fully endorses and supports the February, 1985 U.S. Department of Energy Magnetic Fusion Program Plan as a strategic framework for carrying out the program in the present national circumstances. Broad and rapid dissemination of this Program Plan is recommended.
>
> Fusion is recognized as a potentially abundant and environmentally attractive energy source, which could sustain industrial society in the 21st Century and beyond. The Committee emphasizes that the period 1985-90 is foreseen as a period of intense scientific and technological progress, with TFTR providing experience with plasmas at near-reactor conditions, and technical advances expected in tokamaks, tandem mirrors, stellarators and reversed field pinches."

If there are any questions, don't hesitate to contact me.

Sincerely,

Ron Davidson

Ronald C. Davidson
Chairman
Magnetic Fusion Advisory Committee

vs

Mrs. **Lloyd**. Thank you for your support and for the benefit of your wisdom.

What is the nature of the minimars work, Dr. Davidson, that is planned and how would it differ from our current mirror configurations?

Dr. **Davidson**. As I understand it, the minimars activity is a Reactor Studies Program at Lawrence Livermore Laboratory which has looked at a number of innovative concepts for tandem mirrors, in particular would incorporate the features of axi-cell confinement, both in the central cell and in the end cells and, with certain assumptions, would allow for a relatively compact tandem mirror reactor.

It really is a concept that is evolving and the minimars design is the latest in a sequence of mirror reactor designs.

Mrs. **Lloyd**. Do you have any suggestions for a low-cost ignition device for the United States?

Dr. **Davidson**. I think the first panel summed it up quite well. I do believe that if we are willing to forego some of the engineering and technology objectives that we originally had for TFCX which made it a $1 to $2 billion step, and focused on an ignition step in the $½ billion range, that it is something that the program should pursue with a vigor. I might comment that there are very active design study activities going on at a number of institutions and it is my perception that the DOE is vigorously exploring what the different options are. I would hope sometime in the next 6 to 12 months there might be a further focusing of those options to determine whether or not any of them are practical.

Mrs. **Lloyd**. That's what I was really getting to. Don't you think it's time we did a little bit more focusing instead of all the work we have spread out?

Dr. **Davidson**. I think there are three or four design studies going on at the present time. Those activities have been progressing for the past 6 months and already they are beginning to converge. I believe the program is going through a very necessary phase now. Since we reduced the cost from $2 billion to $½ billion, it's important that the optimum among the concepts in the $½ billion class be examined. I believe that process should take another 6 months or so before preferred options can be selected.

Mrs. **Lloyd**. Thank you very much. Thank you for being with us this afternoon.

We now have our panel No. 2; Dr. Allan Mense, Institute of Electrical and Electronics Engineers. Al is certainly no stranger to this committee because he is a former member of the staff and we still miss him. We're glad to have you with us today. Also, Dr. Peter Staudhammer from TRW and Mr. Paul M. Koloc, who is with us also. Thank you for being with us. We have your statement and you may proceed as you wish. Dr. Mense.

STATEMENT OF DR. ALLAN T. MENSE, INSTITUTE OF ELECTRICAL & ELECTRONICS ENGINEERS, INC.

Dr. **Mense**. Thank you, Madam Chairman. I certainly appreciate having the opportunity to appear before you. I might say that I miss you folks as much as you miss me.

Mrs. LLOYD. I think you improved on your status since you were here last.

Dr. MENSE. Yes, I have. I see the chairman keeps changing names, too.

Let me tell you that today I'm representing the Institute of Electrical and Electronics Engineers. We're the largest engineering society in the world, with over 250,000 members, 200,000 of which live and work in the United States. Approximately 82 percent of our membership is in industry, with 10 percent in various Government laboratories, and 8 percent in academic institutions. If my testimony begins to have a bit of an industrial bent, it's intentional. That's where we come from and that's what we deal with in day to day. I'm certainly pleased to have an opportunity to be here. One of the advantages of appearing at this slot on this program is that everyone before me has really laid an awful lot of good groundwork and I will try not to waste your time by replowing a lot of the issues that they've come forward with, but simply try to focus in on what we perceive to be a few critical questions that have been asked this afternoon.

The three major issues that we think the program is facing right now and which Congress ought to turn their attention to are an ignition and burn experiment and the importance of it; the concern we have for the declining role of fusion in the administration's budget; and the advisability of international cooperation.

First of all, simply put, the need for an ignition and burning experiment is simple: the need for the physics information is now here. We now have a need for that information and we must be forward to do it. One of the questions that I think was posed from the subcommittee to an earlier speaker was what are we doing here with an experiment that everyone is saying is going to cost a $½ a billion or $400 million, when 2 or 3 years ago it was going to cost $2 or $3 billion. What you're doing is buying risks with this lower cost. Let me put it this way: Why is it that machines that we've looked at for the last 4 or 5 years were large and expensive? The reason is because the scientists feel the perceived need to have knob room. We wanted to raise the magnetic fields and lower the magnetic fields. We wanted to put in different kinds of heating. We needed space to do it. This is an Experimental Research Program. We have not honed in on an exact type of device that you can build for a commercial powerplant. When you gave yourself the opportunity to have the knob room and reduce the risk, what it did was cost you a lot of money.

The program has literally been told we're not going to spend that kind of money. When the cannibals are after you, a small rope across a gorge looks like a reasonable risk. I would contend that the program is going through a bit of that right now. There is much higher risk in going with a low-cost approach which only has the capability of operating in a certain parameter regime. If you're willing to buy that risk, then this is the way to go.

I think that the Fusion Program is really fraught with a dilemma. We've pointed out this dilemma before. On one hand we believe that if we don't proceed forward with a clearly perceived end goal of producing energy in a reasonable period of time, that the program will not have a goal.

On the other hand, if we opt for one particular approach for reaching fusion over another, the concern is that you've done away with potentially very good long-term commercial options. It's a real dilemma. I don't think we have any particular insight on this, but what I would say is that I'm more afraid that the near term funding would be cut because we don't have a clearly perceived goal and then we will overlook a promising alternate approach by moving forward aggressively.

In fact, I would say that one of the things that Congress may want the DOE to look at is the idea of taking a number of the compact devices and have them technically administered through a lead laboratory, such as Los Alamos. This is presently done in a technical sense for tokamaks and mirrors right now through Princeton and Livermore. We're not suggesting that you go do it, but this is certainly something that you would want to explore. The advantage being, that if the compact alternate concepts had a lead laboratory kind of status, then they may be able to survive what might prove to be very tight and stringent budgetary times when you're moving forward with a major project.

That's what I would like to say in terms of an ignition experiment. The second point that the Institute wanted me to address was the problem that we see with the administration's lack of concern about the funding of the program. For that, if you will note in table 1, I have provided for fiscal year 1982 through 1986, the funding levels for all of the programs in the Office of Energy Research, which is the area in the DOE in which fusion is funded. You will notice a couple of interesting things. First of all, fusion is the only program which has less as-year-dollars in 1986 than they had in 1982. I don't have inflation or deflation factors in here. I'm talking about as-year-dollars.

I think that what we would like to do is understand the rationale for that. To do that, we might ask what fusion produces. This depends upon the time frame in question. In the medium term, fusion reactor technology development and demonstration will stimulate the country's innovative engineering, materials development, and industrial project management capabilities. In the short and medium term, we will address probably the most challenging scientific problem mankind has tackled. I don't think there's a scientist in this country or any other country that will tell you that the fusion problem is of lesser scientific consequence than what they're working on.

What we have before us here then is a very challenging scientific problem called fusion. Not only are we a worthwhile and strong scientific endeavor, but we have something in addition. We also have a visible, practical, and clearly perceived end goal. We want to produce a method of producing energy which will provide the Earth with power for 1,000 million years. As you know, the Earth will spiral into the Sun before that amount of time is up, so for a physicist that may not be an infinite amount of energy, but for those of us that are engineers that's an infinite amount of energy.

We think that we have a program here that is worthwhile scientifically and technologically. If we are a challenging scientific program, then table 1 shows us that we haven't been funded as though we are a challenging scientific program. If we are a challenging scientific program which also has a clearly perceived end use, then we as an institute would claim that fusion should probably receive more funding than those programs which are scientifically nice but do not have a clearly perceived end use.

As you can see from the funding, that has not happened. I have only picked as examples here programs within the Office of Energy Research. We could go outside of those programs if we wanted to and also see that funding has increased in science areas and fusion has suffered. We think that it's important that the Congress take a look and try to understand why that has been done by the administration.

The third point I would like to address is the idea of international cooperation. First of all, from a scientific point of view, the Institute of Electrical and Electronics Engineers has gone on record before in other hearings as favoring scientific cooperation. The key word here is scientific. I think when we begin to move into the next major phase of the fusion program, however, we're going to be dealing not only with science, but we're going to be dealing with very innovative technology. I don't have to tell the folks here that we've already got a problem in this country with the technology lead times between the time we develop it and the time some other country who has a more focused Government-industry relationship targets that technology and then uses a lower labor rate to bring in a more favorable commercial advantage. I think the Institute would caution that if we move into an international collaboration, that we watch very carefully the technology exchange that may occur.

Three of the points that I would make on this is that in an international cooperation if one is not careful, three bad things can happen. One, you can reduce the U.S.'s spinoff of things, i.e. technologies that spin off back to the U.S. consumer for him to improve his standard of living; second, you're going to reduce the incentive for U.S. industry to be involved and bring their checkbooks; and, third, I think you're going to see a reduced public commitment to the program. I think nationalism is an important ingredient in any formula for a program that's strong and viable. I think that kind of thing gets reduced.

In summary, we would like you to do three things. First, encourage the DOE to initiate an ignition experiment sooner not later. Second, do not allow international cooperation to be built on the basis of technology giveaway, but on equal partnerships in science and education. Third, examine the priority of the fusion funding versus comparable programs of comparable scientific status and readjust this priority, if you see clear to do so.

Let me state quite clearly, that the Institute understands and desires that Congress and the administration exercise restraint in Government spending. Exercising restraint does not mean abandoning initiative. The words budget deficit should not immobilize our efforts to do the right thing. I think that's the key issue here.

To help control the budget deficit, fusion funding must clearly give its fair share. I think we have shown and proven in our testimony from facts not fiction, that fusion has given far more than its fair share. As a very final point, let me say that it isn't the Institute's policy to tell Congress how to do its business. However, we feel it important to emphasize a factor of increasing concern to the technical community.

There has not been an energy authorization bill signed into law since fiscal year 1978. I have a technical note: Obviously, in 1981, the Omnibus Reconciliation Act did in fact do some authorizing, but I mean actually to the floor, let's fight it out and pass the bill. We all understand the reluctance on the part of the Senate to undertake committee debates on an authorization bill only to repeat the same arguments as members of the Appropriations Committee. It certainly looks like double effort of their part. The Institute can't help but think that if there had been a 1985 DOE authorization bill signed into law, that the House Appropriations Committee might not have decimated the fusion budget so severely.

If you will recall, fusion experienced the largest appropriations reduction of any DOE program in fiscal year 1985. The IEEE believes that passing an authorization bill and having it signed into law exercises far more influence than one might at first believe. To establish science and energy as a priority item can only be done through the congressional authorization cycle.

We would like to encourage the committee to pass a bill this year. In exchange, we will try to encourage the Senate to do likewise. We all need the law behind us. Thank you very much.

[The prepared statement of Dr. Mense follows:]

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A CENTURY OF ELECTRICAL PROGRESS

IEEE TECHNICAL ACTIVITIES BOARD/UNITED STATES ACTIVITIES BOARD
ENERGY COMMITTEE

STATEMENT

of

DR. ALLAN T. MENSE

of the

INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS, INC.

on

FUSION ENERGY RESEARCH

Before the

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

HOUSE SCIENCE AND TECHNOLOGY COMMITTEE

March 18, 1985

THE INSTITUTE OF ELECTRICAL AND ELECTRONICS ENGINEERS INC

I am Allan T. Mense, Senior Scientist with the Fusion Science Department of the McDonnell Douglas Astronautics Company and the Vice Chairman of the Institute's Energy Committee. I am appearing before you this afternoon to present testimony on behalf of the Energy Committee of the Institute of Electrical and Electronics Engineers (IEEE). The IEEE is the world's largest engineering society, with nearly 260,000 members worldwide, 215,000 of whom live and work in the United States. Approximately 82% of our membership is employed in industry, 8% in academic institutions, and 10% in the various government laboratories and agencies. The Energy Committee itself has representatives from 32 different technical societies (within the Institute) and 6 different geographical regions. The Committee is vested with the authority to develop the Institute's energy policy and, once that policy is adopted by the Institute, we are one of the major mechanisms by which the policy is voiced in public.

We are pleased to have been given the opportunity to appear before you today and address the very important area of fusion energy development. We continue to believe that fusion possesses the highest potential of all presently known energy sources and wish to commend this Subcommittee for its strong support of the fusion effort over the years. Rather than cover the entire gamut of fusion related activities, we have chosen to address a few major issues which we believe require serious Congressional attention.

The issues are:

1. The need for an ignition/burning core experiment.
2. The declining role of fusion under the Reagan Administration.
3. The advisability of international cooperation.

In addition to these issues, we have included parts of the Institute's testimony from last year as appendix A. (This appendix reiterates a few of the reasons why we believe fusion should be pursued even if one believes we do not presently need additional energy supplies.)

Now let me address the most troublesome problem first.

1. The need for an Ignition and Burning Core Experiment

   The IEEE United States Activities Board, in its most recent position paper on Fusion Power (enclosed as Appendix B) dated November 29, 1984, "recommends that the Department of Energy immediately embark upon the design and construction of the next major evolutionary step in the fusion program, generally referenced to as the 'Burning Plasma Experiment'."

   Let me elaborate on the reason for this position. The fusion program requires one final piece of critical scientific information before it can, with some certainty, develop a practical power producing fusion reactor: the operational characteristics of an ignited and burning plasma core. The present Department of Energy funding levels for fusion preclude the possibility of obtaining this data for the next 10 to 20 years.

   If the program is not allowed to move forward with a certain minimum momentum, then one of the consequences is a gradual but noticeable reduction in trained personnel. When major facilities are not aggressively operated (e.g. TFTR) or constructed (MFTF-B), then people in the program become frustrated with the best people attracted away from fusion. (We are seeing this happen now.) When the best people leave and the operating time of an experiment is reduced, there is less scientific progress. When the Congress and OMB see less progress, then more-often-than-not the result is reduced program funding. This puts the program into an unwise, ever-declining funding spiral. The IEEE Energy Committee believes that if the program is to maintain any semblance of initiative, it must be allowed to move forward to construct facilities for studying an ignited and burning plasma.

Our position on this is thus very much at odds with the Department of Energy's present Magnetic Fusion Program Plan which does not envision a line item for an ignition experiment until 1990 at the earliest. The country will have spent $1.6 billion in additional fusion support before the Department even plans to ask for this experiment. We consider this plan unacceptably long.

2. Why has fusion received reduced funding compared to other major scientific programs within the Administration?

Let us make the case with statistics. Within the Department of Energy's Office of Energy Research (OER), in which the fusion program resides, fusion funding requests, of all the OER programs, have not faired well. Through the present fiscal year (1985), High Energy Physics increased by nearly 50% from $364.5 million in 1982 to $545.6 million in 1985. Nuclear Physics increased by 37% over the same period and Basic Energy Sciences increased from $241 million to $422.6 million or over 75%. In the 1982 to 1985 period, fusion experienced a 7% growth in terms of requested funding. However, due to a $46 million reduction in the 1985 Appropriations Bill, fusion has experienced a 3% overall decrease in funding, the only OER program to have less dollars in 1985 than in 1982.

It appears that 1986 will see the budgets for most non-military programs cut, and it appears that fusion will not be an exception (The $390 million FY'86 requests imply a net program reduction of over 14% since 1982.) See Table 1.

TABLE 1.

BUDGET HISTORY OF SELECTED SCIENCE PROGRAMS

FISCAL YEAR	HIGH ENERGY PHYSICS	NUCLEAR PHYSICS	BASIC ENERGY SCIENCES	FUSION
	(MILLIONS OF DOLLARS)			
FY 82	364.5	133.9	241.2	451.2
FY 83	431.0	137.0	282.4	461.3
FY 84	472.9	163.2	335.6	468.4
FY 85 (est.)	545.6	182.9	422.6	437.0
FY 86 (proposed)	510.1	172.9	406.8	390.0
% change FY'85 to FY'86	-7%	-5%	-4%	-11%

Independent of the magnitude of the fusion budget, one must still try to understand the logic behind the present funding for fusion. To get a handle on this problem, let me review the objectives of the fusion program.

The fusion program has the well-recognized long-term goal of providing the world with essentially limitless energy from sources (deuterium extracted from water) which enjoy no geographic or political boundaries.

The specific objectives and benefits of fusion research depend on the timeframe in question. In the medium-term, fusion reactor technology development and demonstration will stimulate the country's innovative engineering, materials development, and industrial project management capabilities.

In the near-term, fusion research will address the most challenging scientific problems mankind has ever tackled, and if properly administered, provide the country with many important spin-offs. This is the phase which begins to repay the taxpayers for their patience and investment. Thus, it would appear that fusion is not only a strong and worthwhile scientific endeavor, but it also has a clearly visible and practical end goal.

If the Administration, with the approval of Congress, wishes to fund "good-science-for-the-sake-of-good-science," then fusion, being one of those good sciences, should receive its fair share of support.

If, on the other hand, it makes good sense to give increased importance to those areas of good science which also have visible practical end uses, then again fusion also deserves increased funding. In either case it is clear that fusion has been overlooked. We would ask Congress to reverse this trend. I will have more to say about this near the end of my testimony.

Let us proceed to the final troublesome point.

3. The Limits and/or Advisability of International Cooperation in Fusion Research.

Again from our own position paper we state, "the IEEE urges the continuance of broad areas of international cooperation in promoting fusion power research."

Thus we endorse open international scientific collaboration. The U.S. has already had very profitable scientific exchanges and should continue to do so. It is the technology exchange which should be of concern. The reason for the concern stems from the fact that the United States government has not had in the past, and continues to lack a government-industry policy. Meanwhile, other countries, which subsidize their industry to penetrate U.S. markets, make the best of this predicament by using our technology developments as a blueprint for implementation and then beat us by manufacturing products at lower costs. It is hard enough to compete with the present lead times for technology transfer. We need not reduce those times even further. It would be useful for Congress to take some position on this issue and Fusion Energy R&D may be a good program in which to address this concern, particularly since the Department of Energy states quite clearly in their program plan that "the milestones (for fusion) should be interpreted as goals that can be achieved with steady domestic support and significant international cooperation and collaboration." (The underlining is mine). This type of statement says to us that the U.S. cannot do the program (even the most modest one presently proposed) without money and/or facilities from abroad. If so, then the country must be prepared for the consequences:

- reduced technology spin-offs to the U.S. taxpayer (who has already shouldered over $3 billion in costs to date);
- reduced incentives for U.S. industry to participate and bring their own financial contributions;
- reduced public commitment to the national goal of fusion power.

I have briefly covered the primary areas of the Institute's concerns. There are, however, several areas of lesser concern worthy of the Subcommittee's attention. I list them below and can elaborate more fully if necessary.

1. During major funding reductions the universities usually pay a disproportionate price. While this does not appear to be the case in FY '86 for fusion, the Congress should maintain an awareness of the importance of the university role in a successful overall fusion development plan.

2. Both Tokamak and Mirror confinement approaches enjoy the benefit of being technically coordinated by so-called "lead laboratories." It may be advisable for the compact reactor concepts to be technically managed through a lead laboratory. Los Alamos would certainly be a logical candidate for such a position.

3. It has come to the Institute's attention that fewer of the really outstanding students are choosing fusion/plasma physics as a discipline for graduate studies. This is usually a precursor to reduced scientific progress, reduced program momentum, and increased technical frustrations. These are bad symptoms and will ultimately affect program health and stability.

Summary and Final Thoughts

In closing, the Institute is asking Congress to do at least two things:

1. Fund DOE to initiate an ignition experiment sooner than currently planned; it is the way to obtain operational characteristics of an ignited and burning plasma core.

2. Do not allow international cooperation to be built on the basis of technology give-away, but upon equal partnerships in science and education.

Let me state quite clearly that the Institute understands and desires that Congress and the Administration exercise restraint in government spending. To help control the budget deficit, fusion funding must clearly give its fair share. What we have proven in our testimony here today is that fusion has given far more than its fair share. Please help.

As a final point, let me state that, while it isn't the Institute's policy to tell Congress how to do its business, we feel it important to emphasize a factor of increasing concern to the technical community. There has not been an Energy Authorization Bill signed into law since PL95-238 in FY'78.* We all understand the reluctance on the part of the Senate to undertake Committee debates on authorization bills, only to repeat the same arguments as members of the Appropriations Committee. The Institute, however, cannot help but think that if there had been a 1985 DOE Authorization Bill signed into law last year, the House Appropriations Committee might not have decimated the fusion budget so severely. If you will recall, fusion experienced the largest appropriations reduction of any Department of Energy program in FY'85. The IEEE believes that passing an authorization bill and having it signed into law exercises far more influence than one might at first believe. To establish science, and energy in particular, as a priority item can only be done through the Congressional authorization cycle.

We would encourage the Committee to pass a bill this year and, in exchange, we will encourage the Senate to do likewise.

Thank you for giving us your time and attention. If there are any questions, I will be happy to answer them.

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* The Ominbus Reconciliation Act of 1981 did include authorization for FY'82 and proposed ceilings for FY'83 and '84.

Appendix A

WHY DEVELOP FUSION NOW?

- Fusion is Potentially the Best Base Station Power Plant

  To fully realize fusion's ultimate potential as an Energy source, it is important to understand that fusion energy development cannot and should not be linked to energy growth projections or forecasts. The reason is rather straightforward. Old power plants will always have to be retired and replaced with new ones. Fusion should be developed because it has the potential to be the best centralized base station power plant, not just another way to provide power. In other words, fusion has the potential of providing the highest quality energy, and quality has been important in many past developments. There is no reason to expect it not to be so with energy. Thus, we would caution against waiting until the next world energy crisis before deciding (possibly too late) that fusion development is needed. The second caution would be that fusion is not developed until one actually builds the facilities; concepts still on paper are not technologies well in hand.

  With the preceding comments in mind, let me proceed to discuss the other fundamental reasons for developing fusion, even if we believed the country had sufficient other resources to provide us all with a so-called "carefree" energy future.

- Fusion Has the Potential to Provide the Most Environmentally Acceptable Energy Source

  The currently used fuels (primarily fossil) that provide the potential for a "carefree" energy outlook also present serious environmental concerns. The concerns range from the scarcity of water for coal slurry pipelines and/or the need for more railways, to the production of acid rain, exacerbation of breathing related disorders, increasing black lung disease, and enhancing the "greenhouse" effect.

Every other energy source, including uranium, geothermal, hydropower and even solar, has its own peculiar environmental liabilities. Although it is premature to be sure, it is the concensus of the technical community that fusion energy will have substantially less environmental liabilities than competing energy technologies.

o Fusion Research Can Enhance the Nation's Security

The ultimate commercialization of fusion will provide the U.S. with an energy source that can in the short run allow us to export more primitive fuels (e.g. coal) much as we do now with excess food. This is an avenue to improve our balance of payments. In the longer run, fusion reactors will be sold worldwide. Both of these effects will relieve one source of world tension, the uneven geographic distribution of primary fuels. In fact, it has been argued that it is immoral for the U.S. not to devlop and use its high technology energy sources itself and, thus, help to relax the market for fossil fuels so that third world countries (who possess large amounts of strategic materials the U.S. needs) can use these fuels to increase their standard of living. Petroleum and synthetic (fossil) fuels should be saved for use as portable (transportation) fuels for which there are no easy substitutes.

- Fusion Development Contributes to a Sense of National Pride and Well Being

  National pride is a citizen's feelings about his country. Positive feelings contribute to productivity, societal rest, economic progress and the general well being of the nation. The U.S has traditionally been a country whose national pride and self esteem have revolved around improvements by technological innovation. Some examples are the Apollo program and the Space Shuttle. The more challenging and useful the technological advance, the greater the benefit in improved national pride. Fusion is viewed internationally and in the news media as the most challenging of mankind's technical problems. Energy development is obviously useful. A country that accepts this challenge to develop fusion will show recognizable world technology/scientific leadership towards a peaceful goal.

- Enhance Our Economic Picture

  As fusion is potentially a multifaceted source of energy (e.g. electricity production, high temperature heat processing, synthetic fuel/hydrogen production, fissile fuel production), it will provide competition for existing methods and, thus, add diversity to the market place. Diversity leads to market stability, produces economic strength, secures and assures national defense, and provides jobs. The multiplicity of end uses for fusion also reduces the risk of developing a technology that will not have a market. Widespread market penetration of new major energy sources has traditionally taken from 50 to 100 years (e.g., wood giving way to coal); therefore, if fusion is to be commercially available in 50 years, now is the time we must start. Another attraction of fusion is that its development will bring new technology companies into the power plant production market place. This will be good for the utilities and the consumer. Pursuing fusion technology development will create new spin-offs in technology development similar to those of the space program.

o Fusion Development Advanced Leading Edge Technology

Fusion research has attracted some of the best and brightest minds in the country and has drawn the best of students into the science/engineering disciplines. If the program now loses momentum, this drawing of now bright talent will slow. Also, fusion can give back a good name to nuclear energy and help keep science/engineering in the educational forefront.

We hope our brief synopses given in the preceding paragraphs will help you, the members of this Subcommittee, in the continuation of your efforts to move fusion research into a higher national priority. To aid in this, we have included a brief outline of our reasoning as an appendix to this testimony.

For these reasons, we believe that the fusion research community, which enjoys the happy situation of having many excellent ideas relevant to the eventual choice of a superior power source, should be supported at a sufficient level to assure the timely investigation of these relevant matters.

Let me now move on to the second major issue we wish to discuss.

SHOULD THE U.S. FUSION PROGRAM MOVE FORWARD NOW WITH THE DESIGN AND CONSTRUCTION OF AN IGNITION DEMONSTRATION EXPERIMENT?

The IEEE fully endorses beginning work now on the Tokamak Fusion Core Experiment (TFCE). The only experimental data base that is sufficiently developed to allow a major advancement to the engineering stage is that of the tokamak concept. Thus, an advanced Tokamak Reactor is the logical candidate at this time. Funding its development is appropriate at this stage.

While this is an easy position to take, and maybe even a popular one, the Energy Committee did not arrive at it easily. Let us share with you this afternoon some of our reasoning.

Fusion has made good technical progress over the past few years. However, from a programmatic point of view, it is still sitting at the same crossroads where it was back in 1980. It would be easy to say that lack of money was the only culprit in bringing about this stagnation. Actually, it is more appropriate to say that fusion was not able to establish itself as a high enough National priority in the eyes of the Administration to warrant their requesting an increase in funding. In tight budget times, holding the line on spending seems reasonable. The logical question is then, "How have other programs faired?" Some energy programs have done much worse than fusion. On the other hand, a comparison of fusion with its fellow programs within the Department of Energy's Office of Energy Research clearly shows that, since 1982, fusion has faired the least well. For the record, the actual increases are shown below in Figure 1.

	1982	1985	3 Year Change
High Energy Physics	364.5	561	+ 54%
Nuclear Physics	133.2	184	+ 38%
Basic Energy Sciences	241.2	420	+ 74%
Magnetic Fusion	451.2	483	+ 7%

FIGURE 1. Comparison of Office of Energy Research Budgets

The conclusion is inescapable, while fusion is still alive, and spending a great deal of money, it is clearly not considered important enough to be moved ahead. (We might note that Congress has added small but significant amounts of money to Magnetic Fusion each year since 1982.) Since DOE clearly has added money to some research programs, this would indicate that all the other OER programs have a higher priority than fusion. The key question becomes, "Is that the continuing wish of Congress?"

To add money to a program which has no clearly defined goals is certainly not appropriate. But it is here where the program meets a dilemma - what should be the next major near term goal? Fortunately or unfortunately, the answer depends on the expected budget levels over the next decade. But you, as responsible members of Congress, aren't going to write a blank check until you know clearly what the goals are. Unfortunately, the Administration, in its desire to hold the budget down, has prevented the fusion program from voicing its desired goals. Meanwhile the program is technically chomping at the bit. The result - frustration. If the frustration is not abated, then the program stands a good chance of decaying, and the U.S. will have demonstrated again its rare ability to achieve failure when victory is in hand.

Fortunately, within the last month, two review committees, which report to the Department of Energy, have recommended that fusion must have a new lead project. The details of these committee reports (the Magnetic Fusion Advisory Committee and the Energy Research Advisory Board) are covered by other witnesses at today's hearings and will not be reiterated here. This lead project, if selected at this time, can only be a tokamak, and it is with some courage that the fusion community has endorsed this step.

The courage comes from the realization that fusion faces two very real risks. The first risk is that, if the program does not proceed with a path which clearly leads to an energy production goal, Congress (or maybe the Administration itself) will cannibalize the program for funds to use elsewhere. The justification for such an action would be the program's lack of a direct energy relevant focus; i.e., no one will support large fusion budgets on the basis that it produces just plain good science. The questions for Congress are numerous. Would you support fusion on the basis that it is good science? Would you support it at the same or increased budget levels? For how long?

The second risk is a more long term risk, but one with some historical precedent. The risk is that the program will move forward prematurely with one particular approach only to find out in future years that that approach lacks the potential to be economically competitive. Meanwhile, the other backup or alternative fusion approaches will be killed off by lack of funding. Would Congress maintain funding for a balanced base program? What if the lead mainline project overran its cost? From where would the additional monies come?

Both risks are real. The fusion program, due to the tightly contrained budgets of recent years, has been forced to choose. The program appears to believe that the first risk (i.e., near term budgetary instability) is the most threatening and has endorsed moving ahead with a new tokamak - the Tokamak Fusion Core Experiment.

The IEEE endorses this decision, but with the qualification that reasonable funding levels must be maintained in the universities and in the most promising of alternate concepts to help mitigate the long term technical risk. The budget, as proposed, covers this long term risk, but fails to initiate engineering development recommended. The IEEE recommends increasing the budget to include TFCE.

ithin our present understanding of fusion physics, large tokamaks do not appear o be capable of penetrating the electrical power production market place in the arly 21st Century. However, if we're not to move forward with the next major okamak experiment, it would be a little like telling the Wright brothers that, ased upon aerodynamic scaling laws, as known at that time, they should not perform their proof-of-principle experiment (i.e., fly a heavier-than-air craft) because their plane does not extrapolate to a safe, viable, and economic 100-passenger commercial airliner. Fortunately, the Wright brothers moved ahead, proved the principle, and future airliners evolved from their start. It seems unreasonable to believe that fusion, unlike every other technology worked on to date, must give birth to a full blown commercially capable device.

We submit that fusion cannot and should not be asked to answer questions of economic potential at this time in the program's development. We must learn the physics of fusing plasmas. We must learn the engineering of fusion devices. This information will benefit all concerned, even the non-tokamak advocates.

We cannot guarantee that decisions made now will lead us down the easiest path to fusion. All we can say is that technology developments seem to progress most successfully when strong and determined decisions to move ahead have been made. We believe an endorsement by Congress to make the Tokamak Fusion Core Experiment a project line item in the FY 85 or FY 86 budget is an imperative for the continued success of fusion and the advancement of science for mankind.

Thank you again for giving the IEEE this opportunity to testify and I will be happy to try to answer any questions you may have on the fusion program and this testimony.

WHY DEVELOP FUSION

o IT HAS A MULTIPLICITY OF END USES

- i) Electricity production (safe, environmentally acceptable, nearly zero fuel cost.
- ii) High temperature heat processing for industrial and chemical manufacturing.
- iii) Hydrogen production (transportation fuel of 21st Century).
- iv) Breed fuel for LWR nuclear reactors.

o DIVERSITY IS EXTREMELY IMPORTANT

- i) Leads to market stability
- ii) Produces economic strength
- iii) Secures the national defense
- iv) Provides jobs

o THE U.S. HAS SPENT OVER $3 BILLION ON FUSION AND THE PAYOFF TO THE TAXPAYER REALLY ONLY COMES IN THE ENGINEERING DEVELOPMENT AND COMMERCIALIZATION PHASES

- i) Fusion Engineering R&D produces cross-disciplines which will strengthen industrial capabilities.
- ii) As in the space program, spin-offs occur and are implemented into the market place very rapidly (primarily with industrial involvement).
- iii) Commercialization depends on the strength of the market, National needs, many factors we are unable to predict now. No fusion - no choice.

o NEW INDUSTRIES NOT PREVIOUSLY IN THE ENERGY PRODUCTION BUSINESS WILL BE MAJOR PLAYERS IN THE FUSION PROGRAM

- i) These new industries will supplement the present industrial base.
- ii) These newer industries will bring more exacting engineering skills, which will produce a healthy competition in the power production business.
- iii) These new industries have long track records of concern for public safety.

o IF WE DON'T, OTHERS WILL

All world fusion programs have been experiencing almost level funding for the past 2 years. However:

- i) Since 1977, the U.S. fusion budget has increased at a rate less than 5%/year to the present level of $471M.
- ii) Since 1977, Japan's budget increased by over 30%/year to the present level of $300M.
- iii) European budgets increased by nearly 100% over the previous 5-year budget cycle.
- iv) Russians employ almost twice as many fusion scientists as the U.S., and they pursue all major concepts simultaneously.

Page 1 of 3

ENTITY POSITION STATEMENT

345 East 47th Street
New York, New York 10017
(212) 705-7900
1111 19th Street N.W.
Washington, D.C. 20036
(202) 785-0017

Subject:

Fusion Power

"An Entity Position Statement is a document, developed to express an opinion by an IEEE entity on a specific topic, issued in the name of the entity. It is distinguished from an IEEE Position Paper, which is issued in the name of the Institute to express a formal opinion." —From IEEE Policy 14 (1979)

IEEE Entity Initiating the Statement: United States Activities Board (initiated by the Energy Committee)

Need for Issuing:

Intended Use:

Initial Distribution:

The IEEE endorses fusion power as an electrical power generation method using abundant fuels while simultaneously reducing radioactive hazards and environmental impact. Fusion also has the potential for improved operating safety. The IEEE urges the continuance of broad areas of international cooperation in promoting fusion power research. We support funding sufficient for major experiments. The present funding levels, although significant, correspond to restricted research level programs, without adequate provision for the major experiments required to solve the scientific problems that block commercial applications.

The Institute therefore urges adequate funding levels be provided by the federal government for fusion power research and development. The IEEE also recommends that the Department of Energy immediately embark upon the design and construction of the next major evolutionary step in the fusion program, generally referenced to as the "Burning Plasma Experiment".

This is one of a series of positions on energy-related matters produced by the Institute of Electrical and Electronics Engineers (IEEE).

Approval by IEEE Entity

Signature	Name	Title	Date
Russell C. Drew	Russell C. Drew	Vice President, Professional Activities	November 29, 1984

Subject:

Fusion Power

ENTITY POSITION STATEMENT

IEEE STATEMENT ON FUSION POWER

Fusion power is regarded by the IEEE as a highly desirable goal because of its potential advantages: abundant fuel, reduced radioactive hazards, improved operating safety, and reduced environmental impact.

The eventual goal of pure deuterium fusion will provide mankind with a permanent inexhaustible fuel supply. The immediate goal of deuterium-tritium fusion, utilizing lithium as a primary fuel, will provide an adequate fuel supply for energy abundance for many centuries.

The currently-considered most probable form of a fusion reactor is the toroidal or closed magnetic configuration, the tokamak being the member of this class most widely studied. Backups to this approach are the open magnetic configurations usually called magnetic mirrors, compact reactors called reversed-field configurations, and the laser-ignited or inertially confined microexplosive systems also under active investigation.

All methods have their enthusiastic supporters, and many objective observers believe that some form of fusion power will succeed and be economically competitive. It is still too early to determine which approach will prove best.

One of the attractive aspects of fusion is the possible application of fusion-fission hybrid reactors. Either nuclear system by itself suffers neutron scarcity. A combination produces neutron abundance and allows systems of striking properties. Early applications of fusion-driven fissioning systems might provide an alternate source of fissionable fuel for converter reactors, and other hybrid systems offer an important alternative to the fast breeder for exploiting our vast depleted-uranium (tailings from enrichment operations) reserves.

The IEEE is aware and approves international cooperation on the scientific aspects of fusion confinement physics and urges that these programs be encouraged and supported because of the key role of fusion power in the long-term solution of world-wide energy problems. Addressing the situation in the United States, the IEEE fears that the present funding level, although significant, corresponds to a restricted research program without adequate provision for the prompt construction of the next major experiment. Before an experimental power reactor can be built, a scientific experiment must be carried out to maintain a self-sustaining fusion-reacting plasma for a period of time long compared to the characteristic times of all relevant physical processes.

A well-run program to achieve a practical fusion power plant must have a proper balance between scientific research and engineering development. Over the history of the fusion power program, various programmatic reviews have alternately recommended one or the other of these tasks as requiring greater emphasis. The IEEE believes that this distinction has lost some of its importance since the next major scientific experiment, the burning plasma core experiment, will be large enough to require relevant engineering development for itself.

The IEEE believes that this important step can: 1) advance science, 2) provide engineering development, and 3) allow industry to acquire significant experience and play an important role in program management.

The balance that the IEEE believes is important to preserve today is that between moving to the sustained plasma burn experiment in the best available machine (the tokamak today) while maintaining parallel studies of alternative methods and experimental work in alternative machines.

The IEEE believes that adequate national funding is essential to 1) provide strong emphasis on the burning plasma experiment while 2) assuring that, should this major experiment prove more expensive than anticipated, alternate approaches will not be squeezed out. Stretched schedules are preferable to restricting the number of approaches at this time in fusion development. The IEEE also believes that international cooperation should be an effective way to keep work on promising alternatives active.

Mrs. LLOYD. Thank you very much, Dr. Mense. I would like to remind you, it was certainly not the fault of this committee that a bill was not signed into law.

Dr. MENSE. Yes, ma'am. I understand that.

Mrs. LLOYD. Dr. Staudhammer.

STATEMENT OF DR. PETER STAUDHAMMER, MANAGER, ENERGY SYSTEMS, TRW, INC., REDONDO BEACH, CA

Dr. STAUDHAMMER. Madam Chairman, I am Pete Staudhammer. I am manager of energy systems operations at TRW. I thank you very much for the opportunity to be able to talk to you on the subject of really great importance to the energy future of this Nation.

TRW is very deeply involved in the fusion program. Our commitment to the fusion program is evidenced by the fact that we are cost-sharing. We have invested over the past couple of years, $14 million in fusion and we certainly are committed to the promise of fusion for the future of this country.

We recognize that this Congress is faced with a very large Federal deficit; a deficit that impacts the economics of this country. However, in cutting the fusion budget, we are postponing the time when we can harnass fusion energy and thus give ourselves energy and economic stability. In effect, we are trading our economic problems of today for much larger economic problems of the future.

The budget trends during the last few years have had very serious consequences on the rate of progress in fusion. The fiscal year 1985 budget cuts experienced already and the administration's request for 1986 are causing a delay of the fusion breakeven experiments in TFTR, a postponement of the MFTF-B program, and also severe curtailments in the development of many crucial fusion technologies.

Thus, the budget cuts are impacting both leading fusion programs as well as the underlying technologies. These are all major effects and they are all very detrimental to the program.

Let me first address the mirror fusion program. The mirror program continues to look viable with very attractive reactor features. The MFTF-B, which is required to confirm the mirror physics, is now about three quarters complete. Not much can be saved by lowering budgets but an awful lot can be wasted.

With the 1985 budget and with the proposed 1986 budget, the mirror is barely surviving. Any further budget decrease would waste the current investment and the mirror option would likely disappear. Also, within this program, a very constructive industry laboratory relationship has evolved. It is an excellent model for not only fusion, but also for all high technology developments, wherein the scientific capabilities of our laboratories are combined with industrial engineering capability.

As I mentioned, TRW cost-shares in this program with Livermore to the extent of about 30 percent. With the 1985 and 1986 budgets, that partnership can barely survive. If the budgets are any lower in 1986, the partnership would be abrogated on the part of the Government.

Let me take a much broader perspective and look at the program as a whole. We are really perplexed as to why the fusion program is cut at this time. The program is doing very well. With fusion today in a better position than any other time previously, the demonstration of the working fusion reactor by the end of this century is now more promising than it ever had been before. Nothing on the technological side has changed to diminish the promise of fusion to meet this goal by the end of the century. In fact, new ideas have emerged that indicate that the goal can be achieved in less time and with less cost—perhaps with as low as $500 million a year, and be achieved in the next 15 years.

The new development that gives us confidence in reducing the time and cost is the evolution of concepts for small, modular, and inherently safe reactors which can be factory built. Work both at Livermore and Princeton has shown that commercially viable reactors are indeed feasible in the 300- to 500-megawatt range. Dimensions of such a reactor would be smaller than TFTR's today. The small size and lower cost which can now be projected gives us renewed confidence in demonstrating a working reactor within a 15 year time period.

The administration has now drawn up a new magnetic fusion program plan. This was referred to earlier by Ron Davidson. In having reviewed that plan, we at TRW agree generally with the program strategy. But, we would really want to add one more very important goal; namely, the demonstration of a working fusion reactor in a reasonable timeframe. We believe that it is now possible to achieve this goal in approximately 15 years, with the budget is building back in the next 5 years to about $500 million a year. The benefits of achieving this goal are enormous. If commercial viability could be established in 15 years, it would have a major impact on U.S. energy and defense policies. The demonstration of a clean, inexhaustible, and economically competitive source of energy that has a fuel supply from the oceans, which would be available to all nations, would go a long way toward reducing world tension and promoting lasting peace.

The first step in achieving this goal is to proceed with an ignited burning plasma experiment. The fusion community is now working on approaches for small, compact, efficient devices. You've heard a number of people referring to the small, high-field copper systems that could indeed achieve this condition. We believe that the physics base is really here to be able to build that kind of machine. We believe our own estimates that it can probably be done within $200 to $300 million, a far cry smaller than what had been projected earlier.

The question of risk was addressed a little bit earlier. I would like to add to that that it's almost axiomatic that when you have less money, the risk goes up. I think we have to keep in mind that the situation today is quite different than it had been a few years ago. First of all we are setting out to do something considerably less. We are not making a large, superconductive device that operates for long time periods. We are now talking about studying the physics of ignited fusion in a small machine and optimizing things to study that problem alone—not a whole range of other problems. I think we are on a physics base today that is considerably better than it was a few years ago. In all, I would think that with the benefit of our current knowledge base, the attempt at an ignition device probably had more confidence than it has a few years ago, even with larger moneys.

In summary, let me state that I still believe that the fusion program is sound with sound national objectives. The 1985 budget has had very severe effects on the program and the 1986 budget proposal brings the program to its absolute minimum level. Any further cuts would gut the program and would probably discourage industry and would waste investments which have already been made. We are most concerned that the current program is no longer goal oriented nor does it use its previous investments effectively.

In particular, the MFTF-B approach should be completed to confirm the promises of the mirrors.

Also, new concepts for small, modular reactors have given new confidence that a working reactor can be developed in 15 years with budget levels returning to the $500 million per year level. This program can and should be pursued. I believe the DOE ought to stand in front of you and make the commitment that they will in fact have a product coming out of a program that you have funded so well over the past few years. The Nation deserves that we have something which comes out of a program of this importance in which we have put so much money. The first step in doing that development is this ignition reactor that a number of us have referred to. I still believe that the fusion program is promising. It's a good investment in our Nation's future and it deserves the support of Congress. Thank you very much.

[The prepared statement of Dr. Staudhammer follows:]

TESTIMONY OF PETER STAUDHAMMER
MANAGER, ENERGY SYSTEMS
TRW, INC.
REDONDO BEACH, CALIFORNIA

BEFORE THE
SUBCOMMITTEE ON
ENERGY RESEARCH AND PRODUCTION
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

MARCH 18, 1985

MADAM CHAIRMAN AND MEMBERS OF THE COMMITTEE. I AM PETER STAUDHAMMER, MANAGER OF THE ENERGY SYSTEMS OPERATIONS OF TRW. I AM RESPONSIBLE FOR FUSION, ISOTOPE SEPARATION, FOSSIL FUEL COMBUSTION, AND ALTERNATE ENERGY. THANK YOU FOR THE OPPORTUNITY TO TESTIFY BEFORE YOU TODAY ON A SUBJECT OF IMPORTANCE TO THIS NATION'S ENERGY SUPPLY IN THE FUTURE.

TRW HAS BEEN INVOLVED IN PLASMA RESEARCH SINCE 1955 AND WE HAVE MADE SIGNIFICANT CONTRIBUTIONS TO PLASMA SCIENCE AND TECHNOLOGY IN ENERGY, SPACE AND DEFENSE PROGRAMS. TODAY WE HAVE OVER 200 SCIENTISTS AND ENGINEERS WORKING IN THIS HIGH TECHNOLOGY AREA. TRW'S COMMITMENT TO THE NATION'S FUSION ENERGY PROGRAMS HAS BEEN DEMONSTRATED BY THE INVESTMENT OF MORE THAN $14 MILLION OF COMPANY DISCRETIONARY RESOURCES IN FUSION AND PLASMA TECHNOLOGY R&D PROGRAMS DURING THE LAST 10 YEARS.

THE FUSION EFFORT AT TRW IS PRIMARILY ASSOCIATED WITH LAWRENCE LIVERMORE NATIONAL LABORATORY (LLNL). OUR CURRENT PROGRAMS IN SUPPORT OF THE MIRROR FUSION PROGRAM INCLUDE THE DESIGN AND MANUFACTURE OF PLASMA HEATING SYSTEMS AND DIAGNOSTICS FOR THE MIRROR FUSION TEST FACILITY (MFTF-B), SCIENTIFIC SUPPORT TO THE TANDEM MIRROR EXPERIMENTS AT LLNL AND THE OPERATION OF A SYMMETRIC TANDEM MIRROR EXPERIMENT AT TRW. IN ADDITION TO THESE MIRROR FUSION PROGRAMS, WE ARE INVOLVED IN PLASMA HEATING TECHNOLOGY DEVELOPMENT AND FUSION REACTOR ENGINEERING STUDIES. THESE FUSION ACTIVITIES INVOLVE MORE THAN 50 TRW SCIENTISTS AND ENGINEERS, AT FUNDING LEVELS IN EXCESS OF $10 MILLION PER YEAR.

WE RECOGNIZE THAT THIS CONGRESS IS FACED WITH A LARGE FEDERAL DEFICIT. HOWEVER, WE WANT TO MAKE YOU AWARE THAT THE BUDGET TRENDS DURING THE LAST FEW YEARS HAVE HAD SERIOUS CONSEQUENCES ON THE RATE OF PROGRESS IN FUSION. IF THERE ARE ANY FURTHER CUTS, WE BELIEVE THE U.S. FUSION PROGRAM WILL BE BROUGHT TO A POINT OF CRISIS. THE BUDGET CUTS EXPERIENCED, AND THE ADMINISTRATION'S REQUEST FOR FY'86 ARE CAUSING THE DELAY OF THE FUSION BREAKEVEN EXPERIMENTS IN THE TOKAMAK FUSION TEST REACTOR (TFTR) AT PRINCETON, THE POSTPONEMENT OF

THE MFTF-B AT LIVERMORE AND SEVERE CURTAILMENTS IN THE DEVELOPMENT OF CRITICAL FUSION TECHNOLOGIES.

THE POSTPONEMENT OF MFTF-B IS OF PARTICULAR CONCERN TO US AT TRW. ANY BUDGET CUTS TO THE ADMINISTRATION'S FY'86 REQUEST WILL PLACE THE SURVIVABILITY OF MFTF-B IN QUESTION. THE TANDEM MIRROR CONFINEMENT CONCEPT REMAINS SOUND AND IT HAS FEATURES WHICH LEAD TO AN ATTRACTIVE COMMERCIAL REACTOR. WE BELIEVE THE MFTF-B WILL SUCCEED IF GIVEN THE CHANCE. OUR PARTNERSHIP WITH LLNL IN THE MIRROR FUSION PROGRAM HAS PROVIDED US WITH AN OPPORTUNITY TO PLAY A MEANINGFUL ROLE IN THE DEVELOPMENT OF FUSION AND WE ARE VERY PROUD TO BE A PART OF THIS WELL-MANAGED PROGRAM.

WE BELIEVE THAT THE LABORATORY-INDUSTRY PARTNERSHIP WHICH HAS EVOLVED IN THE MIRROR FUSION PROGRAM PROVIDES AN EXCELLENT MODEL FOR THE NATION TO PROCEED WITH THE DEVELOPMENT OF FUSION. IN PARTICULAR, THESE PARTNERSHIPS MERGE THE SCIENTIFIC CAPABILITIES OF OUR NATIONAL LABORATORIES WITH THE ENGINEERING AND MANAGEMENT CAPABILITY OF INDUSTRY. THEY CAN BE IMPLEMENTED WITHIN THE EXISTING FUSION PROGRAM WITHOUT REQUIRING BUDGET INCREASES AND THEY PROVIDE RAPID TRANSFER OF THE TECHNOLOGY BEING DEVELOPED IN THE FUSION LABORATORY TO INDUSTRY. THIS WILL ACCELERATE TECHNOLOGY SPINOFF TO COMMERCIAL MARKETS.

TAKING A BROADER PERSPECTIVE THAN THE MIRROR PROGRAM, WE ARE PERPLEXED BY LAST YEAR'S BUDGET REDUCTION BY CONGRESS AND THE ADMINISTRATION'S FURTHER REDUCTION IN THE FY'86 REQUEST. THESE BUDGET CUTS ARE HAVING A SEVERE IMPACT ON THE ENTIRE NATIONAL FUSION EFFORT AND THEY ARE COMING AT A TIME WHEN TECHNICAL PROGRESS HAS BEEN EXCELLENT AND THE SCIENTIFIC BASIS FOR FUSION IS BEING FIRMLY ESTABLISHED. THE BUDGET CUTS ARE CAUSING LOSS OF BOTH PROGRAM DIRECTION AND FOCUS ON THE ENERGY GOAL. THEY ARE JEOPARDIZING THE PROGRAM'S ABILITY TO CAPITALIZE ON THE LARGE INVESTMENTS ALREADY MADE. THEY ARE CAUSING THE U.S. TO LOSE INTERNATIONAL LEADERSHIP.

THE BUDGET CUTS ALSO AFFECT INDUSTRY INVOLVEMENT IN THE PROGRAM MOST HEAVILY BECAUSE THE BULK OF INDUSTRIAL INTEREST AND MEANINGFUL INVOLVEMENT HAS OCCURRED IN RECENT YEARS AND IS PRIMARILY ASSOCIATED WITH LARGE CONSTRUCTION PROJECTS AND THE ENERGY FOCUS OF THE PROGRAM.

ONE OF THE MOST IMPORTANT RESOURCES OF THE FUSION PROGRAM IS ITS SKILLED TECHNICAL PERSONNEL. FOR MORE THAN THIRTY YEARS THE MAGNETIC FUSION PROGRAM HAS ATTRACTED SOME OF THE BEST SCIENTIFIC AND ENGINEERING TALENT THROUGHOUT THE U.S. THE BUDGET CUTS WILL CAUSE THE LOSS OF KEY PEOPLE AND DIMINISH THE FUSION PROGRAM'S ABILITY TO ATTRACT THE BEST TALENT COMING FROM THE UNIVERSITIES.

THE ADMINISTRATION'S RATIONALE FOR THE BUDGET CUTS APPEARS TO BE THAT THE URGENCY TO DEVELOP NEW SOURCES OF ENERGY HAS DIMINISHED. ENERGY EXPERTS RECOGNIZE THAT IT TAKES MORE THAN TWENTY-FIVE YEARS TO COMMERCIALLY INTEGRATE A NEW SOURCE OF ELECTRIC POWER. THESE 25 YEARS INVOLVE TAKING A NEW POWER-GENERATING TECHNOLOGY FROM THE POINT WHERE COMMERCIAL FEASIBILITY IS DEMONSTRATED TO A POINT WHERE IT IS CONTRIBUTING A SIGNIFICANT FRACTION OF ELECTRICITY PRODUCTION. FUSION IS NOT AN OPTION TO MEET OUR ENERGY NEEDS AROUND THE TURN OF THE CENTURY. IT NEVER HAS BEEN. THE PLAN HAS BEEN TO COMMERCIALLY DEMONSTRATE FUSION AROUND THE TURN OF THE CENTURY SO THAT IT WOULD BE AVAILABLE AS A SIGNIFICANT SOURCE OF ENERGY FOR THE SECOND QUARTER OF THE NEXT CENTURY. I CANNOT UNDERSTAND HOW ANYONE TODAY CAN CONFIDENTLY PREDICT WHAT THE ENERGY NEEDS OF THE U.S. WILL BE IN THE SECOND QUARTER OF THE TWENTY-FIRST CENTURY. NOT DOING THE R&D TO ENSURE AN ADEQUATE SUPPLY OF CLEAN, ECONOMICAL ENERGY IS SIMPLY GAMBLING WITH THE WELL-BEING OF THE NEXT GENERATION OF AMERICANS.

MEMBERS OF THE HOUSE SCIENCE AND TECHNOLOGY COMMITTEE WHO DRAFTED THE MAGNETIC FUSION ENERGY ENGINEERING ACT OF 1980 AND THE MEMBERS OF CONGRESS WHO UNANIMOUSLY PASSED THEIR BILL CLEARLY

UNDERSTOOD THESE POINTS. THE ACT CALLED FOR A FUSION DEVELOPMENT PROGRAM COSTING APPROXIMATELY ONE BILLION DOLLARS PER YEAR FOR TWENTY YEARS TO DEMONSTRATE THE COMMERCIAL VIABILITY OF FUSION ENERGY. NOTHING HAS CHANGED TO DIMINISH THE PROMISE OF FUSION TO MEET THIS GOAL. IN FACT, RECENT PROGRESS AND NEW IDEAS THAT HAVE EMERGED INDICATE THAT THIS GOAL CAN BE ACHIEVED IN LESS TIME AND WITH LESS COST: PERHAPS AS LOW AS $500 MILLION PER YEAR FOR FIFTEEN YEARS.

ONE SUCH NEW IDEA IS THAT BEING PURSUED BY GRANT LOGAN AT LLNL, WHO HAS USED A RATIONALE THAT IS BEING PURSUED FOR ADVANCED FISSION REACTORS AND HAS APPLIED IT TO DEVELOP AN ATTRACTIVE TANDEM MIRROR FUSION REACTOR. HIS IDEA IS TO DEVELOP SMALL-SIZED REACTORS (LESS THAN 500 MWE) WHICH CAN BE FACTORY-BUILT AND SHIPPED TO THE REACTOR SITE. IMPORTANT FEATURES OF THESE SMALL FUSION REACTORS INCLUDE INHERENT (PASSIVE) SAFETY AND STANDARDIZATION TO EXPEDITE LICENSING AND REDUCE CONSTRUCTION TIME. THESE UNITS CAN ALSO BE COMBINED TO TAKE ADVANTAGE OF ECONOMY OF SCALE. THE IMPORTANT POINT IS THAT THESE SMALL, INHERENTLY SAFE REACTORS CAN BE DEVELOPED, DEMONSTRATED, AND COMMERCIALLY INTRODUCED MUCH MORE RAPIDLY AND WITH LESS COST THAN WOULD BE REQUIRED FOR LARGE FISSION OR FUSION REACTORS.

IN PARALLEL WITH THIS EFFORT, PRINCETON PLASMA PHYSICS LABORATORY (PPPL) HAS BEEN EXPLORING PHYSICS WHICH POINTS THE WAY TO SMALLER TOKAMAK FUSION REACTORS WITH ACCEPTABLE CAPITAL AND OPERATING COSTS. OUR ENGINEERS AT TRW HAVE BEEN WORKING WITH SCIENTISTS AT PPPL ON THIS NEW AND PROMISING APPROACH. SPECIFICALLY, WE HAVE FOUND THAT FUSION POWER PLANTS THAT CONSIST OF SEVERAL OF THESE SMALLER TOKAMAK UNITS CAN HAVE ATTRACTIVE ELECTRICITY COST. IF THE THEORETICALLY PREDICTED PHYSICS RESULTS DEVELOPED BY PPPL COULD BE REALIZED, A TOKAMAK IGNITION DEVICE COULD

HAVE SMALLER DIMENSIONS THAN TFTR AND SMALLER, COMMERCIAL REACTORS IN THE 300 MWE TO 400 MWE RANGE WOULD BE FEASIBLE.

I MENTIONED EARLIER THAT THE IDEA FOR SMALL, INHERENTLY SAFE FUSION REACTORS WAS ADOPTED FROM ADVANCED FISSION REACTOR WORK. IF ADVANCED FISSION REACTORS WITH THESE SAME CHARACTERISTICS ARE DEVELOPED AND COMMERCIALLY DEPLOYED, FUSION BREEDERS CAN MAKE FUEL FOR THEM. STUDIES AT TRW AND LLNL HAVE SHOWN THAT FUSION WILL HAVE THE CAPABILITY TO PRODUCE AN INEXHAUSTIBLE SUPPLY OF FISSION FUEL AT PRICES WHICH ARE VERY CLOSE TO TODAY'S URANIUM PRICES.

THE DOE'S DRAFT MAGNETIC FUSION PROGRAM PLAN OF FEBRUARY 1985 OUTLINES THE CURRENT U.S. RESEARCH STRATEGY WHICH IS AIMED AT RESOLVING FOUR KEY ISSUES. THESE ARE: 1) THE IDENTIFICATION OF ACCEPTABLE CONFINEMENT SYSTEMS, 2) EXPLORATION OF THE BURNING PLASMA STATE, 3) DEVELOPMENT OF FUSION MATERIALS, AND 4) DEVELOPMENT OF THE NUCLEAR TECHNOLOGY. THE ESSENCE OF THE PROGRAM STRATEGY IS TO MAINTAIN A BROAD, DOMESTIC R&D PROGRAM WITH EMPHASIS ON ESTABLISHING THE SCIENTIFIC AND ENGINEERING BASIS FOR FUSION. WHILE WE ENDORSE THIS PROGRAM STRATEGY, WE RECOMMEND ADDING ONE MORE IMPORTANT GOAL: THE DEMONSTRATION OF A WORKING FUSION REACTOR IN A REASONABLE TIME FRAME. WE BELIEVE THAT IT IS NOW POSSIBLE TO ACHIEVE THIS GOAL IN APPROXIMATELY FIFTEEN YEARS, WITH FUSION BUDGETS BUILDING BACK IN THE NEXT FIVE YEARS TO EARLIER BUDGET LEVELS WHICH WERE ABOUT $500 MILLION PER YEAR. THE BENEFITS OF ACHIEVING THIS GOAL ARE ENORMOUS. IF COMMERCIAL VIABILITY COULD BE ESTABLISHED IN FIFTEEN YEARS IT WOULD HAVE A MAJOR IMPACT ON U.S. ENERGY AND DEFENSE POLICY. THE DEMONSTRATION OF A CLEAN, INEXHAUSTIBLE AND ECONOMICALLY COMPETITIVE SOURCE OF ENERGY THAT HAS A FUEL SUPPLY (THE OCEANS) AVAILABLE TO ALL NATIONS WILL GO A LONG WAY TOWARD REDUCING WORLD TENSION AND PROMOTING LASTING PEACE.

THE FIRST STEP IN ACHIEVING THIS GOAL OF A WORKING FUSION REACTOR IS TO PROCEED WITH AN IGNITED, BURNING PLASMA EXPERIMENT.

THIS EXPERIMENT WILL SIGNIFICANTLY ADVANCE THE SCIENCE AND TECHNOLOGY BASE OF THE PROGRAM. THE FUSION COMMUNITY IS WORKING ON SEVERAL DIFFERENT APPROACHES FOR SMALLER, LESS COSTLY IGNITION EXPERIMENTS. IN PARTICULAR, WORK GOING ON AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY PLASMA FUSION CENTER AND PPPL IS INDICATING THAT A TOKAMAK IGNITION EXPERIMENT CAN BE BUILT FOR LESS THAN $400 MILLION. THE CURRENT EXPLORATORY EFFORTS SHOULD BE NARROWED DOWN TO ONE OR TWO OPTIONS IN FY'86 SO THAT CONCEPTUAL DESIGNS CAN BE PERFORMED ON THE MOST PROMISING OPTIONS WITH EXISTING PROGRAM RESOURCES. THE CONCEPTUAL DESIGNS CAN PROVIDE THE PERFORMANCE AND COST ESTIMATES NEEDED FOR A DECISION TO PROCEED WITH AN IGNITION EXPERIMENT IN FY'87.

THANK YOU FOR YOUR TIME. ARE THERE ANY QUESTIONS?

TRW BIOGRAPHY

Peter Staudhammer, Manager
TRW Energy Systems Operations

Peter Staudhammer, a TRW executive with more than 26 years of company service, is the manager of TRW's Energy Systems Operations. He is responsible for a wide range of energy research and development programs — including fusion, isotope separation, magneto-hydrodynamic power, and coal combustion.

Prior to his present responsibilities, he was manager of TRW's Advanced Technology Laboratory, where he managed research and development programs in plasma, chemical, and solid state physics and for laser systems and space instruments.

Among the space instrument programs Dr. Staudhammer supervised were the Viking Mars Lander Biology Instruments, the first device to search for life beyond the Earth; ultraviolet spectrometers for the Mariner Jupiter/Saturn spacecraft; gas chromatographs for the Pioneer Venus Lander; the Earth Radiation Budget Instrument; as well as two instruments on board the Voyager spacecraft now in flight to Uranus.

Earlier in his TRW career, he was manager of the Nuclear and Electric Systems Laboratory. Dr. Staudhammer joined TRW in 1959 as a propulsion engineer and worked on design and analysis studies for the Atlas, Titan, and Minuteman propulsion systems. He became manager of the Propulsion Development, where he was chief engineer for the Apollo Lunar Module Descent Engine.

Before joining TRW, he was a senior research engineer at the Jet Propulsion Laboratory, California Institute of Technology.

Dr. Staudhammer attended the University of California, Los Angeles, where he earned his B.S. in electrical engineering in 1955, his M.S. in chemical engineering in 1956, and his Ph.D. in chemical engineering in 1957.

He is the recipient of the Institute of Engineering Advancement Award (for the Viking Mars Biology Instrument) and of the NASA Achievement Award (for the Voyager ultraviolet spectrometer).

Dr. Staudhammer and his wife reside in Rolling Hills Estates, California; they have three daughters. He is active in community affairs, serving as president of the South Coast Symphony, as past president of the South Coast Orchid Society, and as a member of the Board of Councilors of the South Coast Botanic Garden.

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03-06-85

Mrs. LLOYD. Thank you for your very fine statement. Mr. Koloc, you may proceed.

STATEMENT OF PAUL M. KOLOC, PRESIDENT, PROMETHEUS II LTD.

Mr. KOLOC. Madam Chairman, members of the committee, thank you for inviting me to testify. I am Paul M. Koloc, an engineering physicist and president of Prometheus II, Ltd.; our mailing address is Box 222, College Park, Maryland 20740. I have been actively working to introduce more end-user compatible innovative concepts for a speedier and less expensive commercial fusion development program. To that end I patented the most advanced concept, the PLASMAK™ configuration, as well as its intermediate embodiment, the Spheromak.

The introduction of the more advanced concept has been delayed by the lack of engineering physics resources in the Office of Fusion Energy. Such resources would be capable of recognizing refreshing approaches, and providing earlier detection of the engineering consequences of the leading concepts for commercial development.

In addition, the makeup of the Magnetic Fusion Advisory Committee primarily consists of members benefiting from large, Government-supported research programs. Noticeably absent are the engineering representatives of the end-user public utilities. This oversight must be corrected if the program is to keep on track and provide the political basis for the National Plasma Science Program to grow and meet the challenge of these difficult times.

There is no time like the present to unleash the innovative strength of industry by providing a fund of 60 million which would be matched dollar for dollar by industry and would be for the purpose of investigating innovative fusion concepts. This money could be made available by funding the fusion budget at anticipated downstream levels. Review of the concepts could be done in major part by engineering representatives of the public utilities. This would ensure that the effective leadership to develop commercially usable concepts would be present. Although other considerations are necessary, this new direction in the fusion program would incorporate the general trend of the recent Congress toward moving R&D initiatives back to the private sector.

Another new direction of the National Plasma Science Program is toward much lower capital cost experiments. This trend is already well established at facilities working with reversed field pinches such as OHTE, ZT40, and others and compact tofoids including the efficient pressure utilization concepts [EPUC] such as the Spheromak and the commercially attractive PLASMAK™ configuration. Diligent work on these concepts could lead to an experiment which will not only ignite but will also have more attractive engineering characteristics for commercial use. Reliance on international cooperation may not be necessary to generate an early ignition experiment.

Because of differences in patent law from country to country and the need for American industry to protect its trade secrets, such international endeavors generate very time consuming and expensive negotiations. Since fusion development is set squarely in the center of the greatest commercial enterprise imaginable, this makes the protection and payback necessary for the serious commitment of American industry very tenuous. There is also the tendency for cooperative international efforts to school around the older and more obsolete concepts.

The engineering approach to compare the various fusion machines is to consider each machine's ability to transmit great pressure to the thermonuclear plasma. Each level of machine sophistication represents the physical embodiment of a more complete set of plasma physics principles.

The mirror incorporates the notion that a vacuum magnetic field can confine a plasma. The simplest coil generated field is solenoidal with constricted ends. Unfortunately, strong pressure enhancement will squirt plasma out the ends like the results obtained by squeezing a full tube of toothpaste.

The next level the Stellarator resulted from elongating the plasma tube and closing the ends together to form a loop. It was necessary to add at least one layer of thick windings that traveled the long way around the loop. Adding more pressure constricted and lengthened the plasma column, thereby triggering a squirming and disruption.

Then came the tokamak with the principle that plasma can carry a current which generates a self-confining field. The outward pressure generated by the self field has to be balanced by a network of vertical field coils. The squirming problem is so intense that another set of much more powerful toroidal field coils must be present. These coils produce little plasma pressure but do generate a strong crushing force on a central steel plug.

The Spheromak captures this great crushing force and applies it directly to the plasma by generating the toroidal field with plasma currents. Consequently, the pressure on the toroidal axis of a Spheromak is 10 times that of the bounding shell pressure, which in turn is about 20 times better than the analogous pressure ratio found in tokamaks. True Spheromaks achieve the remainder of the outward pressure balance through currents flowing in a solid conducting shell.

The PLASMAK(tm) configuration replaces the solid conducting shell with a plasma mantle and an energetic electron current sheet. The exterior of the plasma mantle is supported by a blanket of ordinary gas. Therefore a PLASMAK can be mechanically compressed by increasing the gas blanket pressure. Adiabatic heating generated by increasing the blanket pressure to 125 atmospheres would be sufficient to generate a commercial breakeven burn in deuterium tritium. However, in a first proof of principle test it is better to burn pure deuterium. Stored deuterium is not inherently radioactive and a PLASMAK compression to 2,000 atmospheres is all that is required to commercially ignite this fuel. The biggest advantage of PLASMAK(tm) technology is its potential to burn hydrogen boron. The great pressures needed to ignite this neutronless fuel appear reachable. Once the fuel core is ignited, the liquid density gas blanket will serve to continue compression heating, ensuring efficient burn. This is accomplished through its absorption and rethermalization of the radiated fusion heat. Electrical power production with 90-percent efficiency will be achieved by expanding the fusion heated blanket directly into an inductive MHD generator.

The expected experimental costs and turn around time are greatly reduced. The capital cost of forming viable PLASMAK's in atmospheric pressure gases is negligible, when compared to the cost of the equivalent tokamak apparatus such as vacuum systems, magnetic coils, power supplies and so forth. The cost in time and money to generate the PLASMAK in air is 1 year and $350,000. To generate the plasmoid successfully in deuterium, do a compression to ignition and commercial breakeven burn would be $4 or $5 million with an additional 2 years. Prometheus is in various stages of discussion of this scenario with several leading high technology corporations.

In conclusion, we would strongly urge the repeal of the unwritten slogan "Fusion Beyond Our Lifetime." Let us also repeal official projections of the cost of fusion development based on obsolete approaches. We are now formulating the National Plasma Science Program. Let's take another look at the possibilities before we jump.

Thank you for your strong support of a viable program to achieve fusion.

[The prepared statement of Mr. Koloc follows:]

TESTIMONY BEFORE THE

HOUSE SCIENCE AND TECHNOLOGY COMMITTEE
ENERGY RESEARCH AND PRODUCTION SUBCOMMITTEE

on the

FY 1986 MAGNETIC FUSION ENERGY BUDGET

by

Paul M. Koloc

President , Prometheus II Ltd.

March 11, 1985

Madam Chairman, Members of the Committee, thank you for inviting me to testify before you today. I am Paul M. Koloc, an engineering physicist and president of Prometheus II, Ltd., Bx 222, College Park, MD 20740. I have been actively working to introduce more end-user compatible innovative concepts for a speedier and less expensive commercial fusion development program. To that end I patented the most advanced concept, the PLASMAK(tm) configuration, as well as its intermediate embodiment, the Spheromak.

The introduction of the more advanced concept has been delayed by the lack of engineering physics resources in the OFE. Such resources would be capable of recognizing refreshing approaches and providing earlier detection of the engineering consequences of the leading concepts for commercial development.

The makeup of the MFAC committee primarily consists of members benefiting from large, government-supported research programs. Noticeably absent from the committee are the engineering representatives of the end user Public Utilities. This oversight must be corrected if the program is to keep on track and provide the political basis for the National Plasma Science Program to grow to meet the challenge of these difficult times.

There is no time like the present to unleash the innovative strength of industry by providing a fund of sixty million dollars which would be matched dollar for dollar by industry and would be for the purpose of investigating substantially unfunded innovative fusion concepts. This money could be made available by funding the fusion budget at anticipated downstream levels. Review of the concepts could be done in major part by engineering representatives of the Public Utilities. This would ensure that the effective leadership to develop commercially usable concepts would be present. Although other considerations are necessary to avoid conflict of interest and the like, such a new direction in the fusion program would incorporate the general trend of the recent Congress toward moving the initiatives for R & D back to the private sector.

This economy would also clarify the need for tough decisions that might be otherwise postponed. The country has a need for a broad spectrum of current plasma physics resources that might be affected by such decisions. Specifically, the Strategic Defense Initiatives would benefit by any talent displaced from affected fusion programs.

Clearly the new direction of the National Plasma Science Program is toward much lower capital cost experiments. This trend is already well established at facilities working with reversed field pinches and compact toroids. These include OHTE, ZT40 and the Efficient Pressure Utilization Concepts (EPUC) such as the Spheromak and the commercially attractive PLASMAK(tm) configuration. Diligent work on the RFP's and EPUC's could lead to an experiment which will not only ignite but will also have more attractive engineering characteristics for commercial use. Reliance on international cooperation may not be necessary to generate an early ignition experiment.

Because of the differences in patent law from country to country and the need for American industry to protect its trade secrets, such international endeavors generate very expensive negotiations. Since fusion development is set squarely in the center of the greatest commercial enterprise imaginable, this makes the protection and payback necessary for the serious commitment of American industry very tenuous. There is also the tendency for cooperative international efforts to school around the older and more obsolete concepts.

The engineering approach to compare the various fusion machines is to consider each machine's ability to transmit great pressure to the thermonuclear plasma. Each level of machine sophistication can represent the physical embodiment of a more complete set of plasma physics principles.

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After this topological implementation came the tokamak, with the principle that plasma can carry a current which generates a self compressing (confining) field. However, the outward pressure generated by the self field had to be balanced by a network of "vertical field coils". The squirming problem is so intense that a much more powerful toroidal field coil must be present. These coils produce essentially no plasma pressure but do generate a strong crushing force on a central massive steel plug.

The Spheromak captures this great crushing force and applies it to the plasma by generating the toroidal field with plasma currents. Consequently the pressure on the toroidal axis of a Spheromak is ten times that of the bounding shell pressure, which in turn is about twenty times the analogous pressure ratio found in tokamaks. True Spheromaks achieve the remainder of the outward pressure balance through currents flowing in a solid conducting shell.

The PLASMAK(tm) configuration replaces the solid conducting shell with a plasma Mantle and an energetic electron sheet current. The exterior of the plasma Mantle is supported by a blanket of ordinary gas, and therefore a PLASMAK(tm) plasmoid can be mechanically compressed by increasing the gas blanket pressure. Adiabatic heating generated by increasing the blanket pressure from one atmosphere to 125 atmospheres would be sufficient to generate a commercial breakeven burn in deuterium tritium. However, in a first proof of principle it would be more convenient to burn pure deuterium, because stored deuterium is not inherently radioactive and a compression to two thousand atmospheres is all that is required to commercially ignite this fuel. The big advantage of PLASMAK(tm) technology is its potential to burn hydrogen boride eleven. The great pressures to ignite this neutronless fuel appear reachable. Once the fuel core is ignited, the liquid density gas blanket will serve to continue compression heating, ensuring efficient burn through its absorption and rethermalization of the radiated fusion heat. Electrical power production with a ninety percent efficiency will be achieved by expanding the fusion heated blanket in an inductive MHD generator.

Since the capital cost of forming viable PLASMAK(tm) plasmoids in atmospheric gases is negligible when compared to the equivalent vacuum chamber, magnetic coils and power supplies, etc. of a tokamak, the expected experimental costs and turn around time are greatly reduced. The cost in time and money to generate the PLASMAK(tm) plasmoid in air is only one year and three hundred fifty thousand dollars. To generate the plasmoid successfully in deuterium and do a compression to ignition and commercial breakeven burn would require as much as four or five million dollars and an additional two years. Prometheus is in various stages of discussion of this scenario with several leading high technology corporations.

In conclusion, we would strongly urge repeal of the unwritten slogan "Fusion Beyond our Lifetime". Let us repeal official projections of the cost of fusion development based on obsolete approaches. We are now formulating the National Plasma Science Program. Let's take another look at the possibilities before we jump.

Thank you for your strong support of a viable program to achieve fusion. I would be pleased to answer any questions.

Mrs. LLOYD. Thank you, Mr. Koloc.

Dr. Mense, do you feel there's a point of diminishing return for R&D on alternative and advanced magnetic fusion confinement systems unless we have the guidance of a low-level burn experiment?

Dr. MENSE. Yes, I think it was mentioned earlier today that the development of technology and probably to a certain extent, the development of science with no clearly perceived direction is not always the most fruitful path to follow, particularly when dollars are tight. From that standpoint, to have a lead program available which is dictating some of the technological requirements, is probably the most cost-effective way to go.

Mrs. LLOYD. Dr. Staudhammer, would it be correct to state after reviewing your testimony, that the Fusion Program should consider three primary goals: Good science; demonstrating sustained plasma ignition; and, developing an integrated system and industrial base leading to a product? If so, what should be our near term and our high priorities?

Dr. STAUDHAMMER. The near term high priority probably has to be the next logical step being ignition. I think a number of people have talked about this. There have been a number of studies of that that have concluded that ignition was the right thing to do. The new developments which I described give us a lot of encouragement in that direction is that I think we can approach that with considerably fewer dollars and at a much smaller scale; a much more manageable scale than we had talked about before. Of course, we recognize that we are focusing on this subject specifically. I think one thing that is truly missing in the program is that it has to have some kind of a product; something that we are all going toward. To me, it's almost unimaginable not to have a product at the end. If the president of TRW would hear me advocate that program, he would roll me out of his office. I don't believe that any industry could accept a program and fund it without having some very specific end goals. I think there is an end goal here. The end goal is a very practical one and the Fusion Program has to incorporate it. The program plan has to be to support that. I'm not saying that we can now conclude exactly how the fusion scenario will go over the next 20 years. I think we need to have a plan that at the current state of knowledge, focuses the effort toward the ultimate application. That is basically missing from the program today.

Mrs. LLOYD. Thank you. Mr. Koloc, what do you believe would be the best way for us to achieve ignition so that it is applicable to commercial reactors?

Mr. KOLOC. I'm basically advocating using devices in which you can generate the greatest pressure. If Tokamak is the only machine in the approach described today which is absolutely the best way to go. We have—we can generate very high density plasmas with many other devices—private industry, General Atomic, for example, certainly believes that and they're investing money in that area.

I think that the best way is to really to increase the amount of money available for study of fusion and one of the ways to do that is this plan where you actually let industry match funds which are contributed by the Government dollar for dollar, and that will basically double the money. If you go ahead and reduce the budget this year, we could still end up with more money.

Industry wants to make an investment. They will work for something they think is going to work in the long run. One of the first things they would want to do is find a concept that they could get to an ignition test in a very short period of time. Open the door to industry to bring them in on an initiative basis not just simply to be subcontractors for this or that, and I think you will find that we will get to ignition in a very short period of time.

Mrs. LLOYD. Do you have any suggestions to further encourage the private initiatives?

Mr. KOLOC. I believe that money should be made available to joint venturing with industry basically. Something on the order of about $60 million is what I think it would require. The other thing of course is that it wouldn't really—the joint venturing wouldn't only be in terms of the money contributed, but they would be able to work with the universities and with the national laboratories. There's a number of quite attractive things going on even in the universities. The University of Maryland has a high density spheromak experiment going, as does Los Alamis which also has the interesting ZT40. I think that Dr. Ohkawa's experiment is certainly worthy of sharing in that, too.

Mrs. LLOYD. Thank you very much.

We appreciate all of our witnesses today and all of the testimony. Mr. Fawell, have you any questions?

Mr. FAWELL. Dr. Mense, to perform the essential fusion R&D programs outlined in your statement and to facilitate international cooperation on an equal partnership, what should be the fiscal year 1986 budget for magnetic fusion energy development?

Dr. MENSE. I'm not a proponent of taking a budget and increasing it so fast that you can't spend the money wisely. I would think that if we would freeze the budget at the 1985 level of $437 million, that in itself would be an adequate move and an adequate balance between budget constraint and the desire to have a program that will not lose its forward momentum.

Mr. FAWELL. Thank you.

Mrs. LLOYD. Thanks to all of our witnesses and, if there are no further questions, the subcommittee stands adjourned.

[Whereupon, at 5:25 p.m., the subcommittee recessed to reconvene at the call of the Chair.]

APPENDIX I

DON FUQUA, Florida, CHAIRMAN

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(202) 225-6371

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General Counsel
JOYCE GROSS FREIWALD
Republican Staff Director

May 9, 1985

Ms. Martha Hesse Dolan
Assistant Secretary for
Management & Administration
U.S. Department of Energy
Washington, D.C. 20585

Dear Ms. Dolan:

We regret that our subcommittees had to cancel the March 11 hearing which had been scheduled for the purpose of reviewing the Fiscal Year 1986 budget request for the Departmental Administration and Supporting Services activities.

In order to assist us in completing our hearing records, we would appreciate it if you would provide responses for the record to the attached questions pertaining to those programs.

Thank you for your help with our oversight efforts.

Sincerely,

Don Fuqua
DON FUQUA
Chairman
Committee on Science & Technology

Marilyn Lloyd
MARILYN LLOYD
Chairman
Subcommittee on Energy
Research & Production

FL:Gdj
Attachment

Questions for the Record
FY 1986 Department of Energy Budget Request
Departmental Administration & Supporting Services Activities

Departmental Administration

1) With regard to the funding table on page 219 in Volume 5 of the FY 1986 budget justification, please provide an updated version of this table which reflects the current organizational structure at the Department. In addition, please indicate which activities within the account will absorb the $18,251,000 of "adjustments" and "savings from Management Initiatives" which are to be applied against the appropriations.

2) The FY 1986 budget request includes a proposed 5% cut in salaries and benefits. Should this proposed 5% cut be rejected by the Congress, how do you plan to fund current salaries and benefits? Would funding for program activities have to be reduced? Do you anticipate the need for a supplemental appropriations request to provide these funds?

3) The FY 1986 request for Management and Administration reflects a net increase of $11,584,000 over the FY 1985 appropriations. The Department's budget justification indicates that most of this increase is to cover the cost of new computer equipment and to provide for new initiatives in the areas of computer systems consolidation. Please provide a detailed description of these costs and initiatives.

4) On page 256 of the budget justification, it is indicated that the Office of Management and Administration is planning to prepare three documents: an official DOE history of Federal energy policy and programs; "U.S. Civilian Nuclear Power (1954-1981);" and "Nuclear Weapons Test Policy." Please provide the anticipated completion date for each of these documents.

5) Included in the budget request for the Office of Minority Economic Impact is a reduction to eliminate the Cooperative Development Energy Programs with energy industries/minority businesses/minority college-universities. Please identify the programs which are currently in effect, but which are targeted for termination, and provide the rationale for terminating these activities.

Supporting Services

1) In-House Energy Management: The FY 1986 request for this program is $19,400,000 more than the FY 1985 appropriations, with $9,000,000 of the increase identified for the continuation of Project Numbered 85-A-606, Cogeneration Facility, Brookhaven National Laboratory, Upton, New York. Please provide a status report on this project, including a description of anticipated benefits to be realized as a result of completing and operating the facility.

2) Technical Information Management Program: This program is responsible for the management of all DOE research results through on-line data bases

and is carried out at a centralized information facility (Technical Information Center) located in Oak Ridge, Tennessee.

The FY 1986 request of $10,000,000 is net of $6,000,000 which is identified as being required for the program, but which has-been proposed for collection from users. Further, the budget justification states that a study will determine the most appropriate method for collecting the user fees.

(a) Has the study been completed? If so, how is the collection of these user fees to be accomplished?

(b) Please describe the _specific functions_ performed by the TIMP for which the costs would qualify for recovery through user fees.

(c) What costs will be associated with the administering of a user fee system?

(d) Please list all the anticipated users (including DOE program offices) which will be required to reimburse the DOE for services performed by the TIMP. To your knowledge, have the potential government users budgeted for user-fee costs in FY 1986?

(e) Will the TIMP functions associated with monitoring the research efforts of other countries be affected by the user-fee system?

(f) How will the implementation of a user-fee system affect the timeliness of providing research results to users? Has the Department received any comments from user agencies regarding the implementation of a user-fee system? If so, please provide a summary of these comments.

(g) What will be the impact on the TIMP if the full $6,000,000 is not recovered in FY 1986? Will such a situation necessitate a reduction in funding for the program? What specific activities in the program would be affected? How will such a situation affect the future of the TIMP overall?

Management Initiatives

The following questions pertain to the "Management Initiatives" described in the FY 1986 budget justification as being "designed to improve the efficiency and effectiveness of Departmental management systems, to take advantage of economies available through increased or improved utilization of private sector capabilities, and to place the Department's financial systems on a more business-like basis."

1) Please provide current information on the implementation status of each of the eleven (11) management initiatives (Administrative Organization Management; Administrative Systems; Administrative Process/Field Structure; Payroll/Personnel; Procurement Reform; Cash Management'; Financial Systems Reforms; Printing/Publication; Travel Management; GS 11-15 Bulge Reduction; and Reduced SLUC Payments). Please include the following information:

a) The total "saved" in FY 1984;

b) Estimated "savings" for FY 1985;

c) Estimated "savings" for FY 1986;

d) In what ways do each of the initiatives relate to recommendations contained in the report entitled "President's Private Sector Survey on Cost Control" and the OMB report entitled "Management of the United States Government - 1986." Please refer to specific recommendations in these reports, when possible.

2) Please describe any new data which has been obtained or new developments which have arisen on review of the implementation of these initiatives.

Department of Energy
Washington, DC 20585

June 10, 1985

Honorable Don Fuqua
Chairman, Committee on Science
 and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

Enclosed are the answers to questions you submitted with your May 9 letter in regard to the March 11 hearing that was canceled. This was a joint subcommittee hearing for the purpose of reviewing the Fiscal Year 1986 budget request for the Departmental Administration and Supporting Services activities.

The enclosed responses for the record also have been sent to the Subcommittee staff.

If you have any questions, please call Cathy Hamilton of my staff on 252-4277. She will be happy to assist you.

Sincerely,

Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

Department of Energy
Washington, DC 20585

June 10, 1985

Honorable Marilyn Lloyd
Chairman, Subcommittee on Energy
Research and Production
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Madam Chairman:

Enclosed are the answers to questions you submitted with your May 9 letter in regard to the March 11 hearing that was canceled. This was a joint subcommittee hearing for the purpose of reviewing the Fiscal Year 1986 budget request for the Departmental Administration and Supporting Services activities.

The enclosed responses for the record also have been sent to the Subcommittee staff.

If you have any questions, please call Cathy Hamilton of my staff on 252-4277. She will be happy to assist you.

Sincerely,

Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

QUESTIONS AND ANSWERS

Relating to the

FY 1986 AUTHORIZATION REQUEST DEPARTMENTAL ADMINISTRATION AND SUPPORTING SERVICES

From the

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

Of the

HOUSE SCIENCE AND TECHNOLOGY COMMITTEE

DEPARTMENTAL ADMINISTRATION

QUESTION: With regard to the funding table on page 219 in Volume 5 of the FY 1986 budget justification, please provide an updated version of this table which reflects the current organizational structure at the Department. In addition, please indicate which activities within the account will absorb the $18,251,000 of "adjustments" and "savings from Management Initiatives" which are to be applied against the appropriations.

ANSWER: An update of the table on page 219 in Volume 5 of the DOE FY 1986 budget justification is attached. There are two differences from the original table. First, the FY 1985 figure for General Counsel has been reduced by $137,000 to reflect the transfer of travel and contracted services to the Economic Regulatory Administration. Second, the new table shows the consolidation of External Affairs with Congressional Affairs.

In FY 1986, the total Departmental Administration request will be reduced by $18,251,000 by use of prior year balances and other adjustments, and Management Initiative savings. The Department proposes to finance $12,251,000 of its FY 1986 request by using prior year unobligated balances of $8,501,000 and by reductions in the number of FTEs resulting in additional savings of $3,750,000. It will not be possible to specifically identify the sources of the FTE related savings until ongoing reviews are completed. An additional $6,000,000 will become available as a result of Management Initiative savings from Inspector General audits and from use of prior year deobligations. Identification of the activities which will incur these savings will not be possible until sometime during FY 1986.

[illegible]

FISCAL YEAR 1986 CONGRESSIONAL BUDGET REQUEST

SUMMARY OF ESTIMATES BY APPROPRIATION BY MAJOR ACTIVITY

DEPARTMENTAL ADMINISTRATION

BUDGET AUTHORITY IN THOUSANDS OF DOLLARS

	FY 1984 Actual	FY 1985 Estimate	FY 1986 Request
Office of the Secretary............	1,143	1,433	1,478
Board of Contract Appeals..........	341	377	371
General Counsel....................	11,760	9,855	9,784
Inspector General..................	14,670	21,617	20,619
Management and Administration......	131,005	130,024	141,608
Operations Offices.................	93,899	89,447	90,059
Minority Economic Impact...........	1,674	2,166	1,827
Policy Analysis and System Studies.	6,461	6,876	7,596
Congressional, Intergovernmental and Public Affairs...............	6,470	4,131	5,193
International Affairs..............	3,031	3,991	4,550
Policy and Management..............	780	795	708
Cost of Work for Others............	116,191	115,704	149,110
Subtotal, Departmental Administration..................	387,425	386,416	432,903
Miscellaneous Revenues.............	-106,895	-128,151	-161,743
Revenues Not Associated with Cost of Work............................	-69,848	-91,308	-82,485
Subtotal, Departmental Administration	210,682	166,957	188,675
Addback of Proposed Rescission....	---	2,786	---
Use of Prior Year Balances and Other Adjustments...............	-25,000	-29,053	-12,251
Savings from Management Initiatives.	---	-6,375	-6,000
Total, Departmental Administration..	185,682	134,315	170,424

Departmental Administration

QUESTION: The FY 1986 budget request includes a proposed 5% cut in salaries and benefits. Should this proposed 5% cut be rejected by the Congress, how do you plan to fund current salaries and benefits? Would funding for program activities have to be reduced? Do you anticipate the need for a supplemental appropriations request to provide these funds?

ANSWER: The FY 1986 budget request for Departmental Administration has undergone extensive review. The request for compensation and benefits has been priced assuming a 5% pay reduction. If the Congress does not approve the five percent pay cut, we will attempt to cover the additional salaries and benefits by either a reprogramming action or a supplemental appropriation. We do not anticipate any reductions in program levels to accommodate the increased costs.

DEPARTMENTAL ADMINISTRATION

QUESTION: The FY 1986 request for Management and Administration reflects a net increase of $11,584,000 over the FY 1985 appropriations. The Department's budget justification indicates that most of this increase is to cover the cost of new computer equipment and to provide for new initiatives in the areas of computer systems consolidation. Please provide a detailed description of these costs and initiatives.

ANSWER: The requested increase for computer equipment will provide funding for conversion of lease to purchase of disk storage equipment and the replacement of obsolete equipment.

The increases for computer systems will support the development of new systems as well as the consolidation of major support systems such as the Departmental Integrated Standardized Core Accounting System (DISCAS) and the Payroll Personnel System (PAY/PERS).

Question for the Record
FY 1986 Department of Energy Budget Request
Executive Secretariat

QUESTION: On page 256 of the budget justification, it is indicated that the Office of Management and Administration is planning to prepare three documents: an official DOE history of Federal energy policy and programs; "U.S. Civilian Nuclear Power (1954 - 1981)"; and "Nuclear Weapons Test Policy." Please provide the anticipated completion date for each of these documents.

ANSWER: Volume III of the History of the U.S. Atomic Energy Commission 1953-1960 (a component of the official history of the Department of Energy and its predecessor agencies, and its policies and programs), "U.S. Civilian Nuclear Power (1954-1981), and "Nuclear Weapons Test Policy" are all scheduled for completion during FY 1986.

Minority Economic Impact

QUESTION: Included in the budget request for the Office of Minority Economic Impact is a reduction to eliminate the Cooperative Development Energy Programs with energy industries/minority businesses/minority college-universities. Please identify the programs which are currently in effect, but which are targeted for termination, and provide the rationale for terminating these activities.

ANSWER: The Cooperative Development Energy Program (CDEP), initiated in 1983, was designed to proceed through phases of planning and development to full implementation. The funding support by the Office of Minority Economic Impact in each successive phase was to be negotiated based upon program accomplishments and funding support from other sources. Each project was reviewed independently and a decision made for continued funding. Two of the four initial projects will receive some Minority Economic Impact funding through Fiscal Year 1985.

Two CDEP programs are currently funded. Central State University in Ohio has agreements with Dayton Power and Light Co., Monsanto Corporation, the NCR Corporation, Price-Waterhouse, U.S. Army Corps of Engineers, Wright-Patterson AFB, nine minority business enterprises, the Lincoln Heights and Urban Crest communities, and State planning agencies. Fort Valley State College in Georgia has agreements with the Atlanta Gas Light Company, Georgia Power Company, Oglethorpe Power Corporation, Fort Valley Utilities Commission, Exxon Corporation, Albany Power Company, Cities Service Gas and Oil of Ohio, ten Electric Membership Cooperatives in Georgia State, Oak Ridge National Laboratory, Lawrence Livermore Laboratory, and the Hoover Corporation.

Our plan is that the success of the current programs would serve to entice stronger private sector support of these type activities and eliminate the need for Federal funds. Minority Economic Impact will continue to make its staff available as appropriate to assist the two current projects and similar new activities under its broad role of management and technical assistance to minorities.

IN-HOUSE ENERGY MANAGEMENT

QUESTION: The FY 1986 request for this program is $19,400,000 more than the FY 1985 appropriations, with $9,000,000 of the increase identified for the continuation of Project Numbered 85-A-606, Cogeneration Facility, Brookhaven National Laboratory, Upton, New York. Please provide a status report on this project, including a description of anticipated benefits to be realized as a result of completing and operating the facility.

ANSWER: The total estimated cost of the Brookhaven cogeneration facility is $20.0 million. In FY 1985, $1.6 million was appropriated to begin design of this project. A design contract was awarded in November 1984. The design firm is presently analyzing the life-cycle economics of two alternate thermodynamic cycles (gas turbine topping cycle versus combined cycle). Completion of Title I design is scheduled for July 1985 and completion of Title II design is scheduled for March 1986. The $10.6 million requested for FY 1986 will allow continuation of Title II design of the gas turbine, start of physical construction of the facility, order of long lead items, and provision for a portion of Title III design.

When completed and operating, the Brookhaven cogeneration facility will save the Department approximately $5.2 million annually in operating costs, and approximately 58,000 barrels of oil equivalent.

TECHNICAL INFORMATION MANAGEMENT PROGRAM

QUESTION: Has the study been completed? If so, how is the collection of these user fees to be accomplished?

ANSWER: The study has not been fully completed. Several options have been identified and are under review.

QUESTION: Please describe the specific functions performed by the TIMP for which the costs would qualify for recovery through user fees.

ANSWER: Tentatively, all TIMP activities are being examined for user fee financing.

QUESTION: What costs will be associated with the administering of a user fee system?

ANSWER: Depending on the option finally selected, costs may range from minimal up to an annual operating cost of $1.5 million.

QUESTION: Please list all the anticipated users (including DOE program offices) which will be required to reimburse the DOE for services performed by the TIMP. To your knowledge, have the potential government users budgeted for user-fee costs in FY 1986?

ANSWER: TIMP serves directly all DOE program offices and approximately 6,000 DOE contractors, and, therefore, the costs would be recouped from those users. No FY 1986 funds have been specifically budgeted to provide for user fee charges. However, we are examining alternatives for funds sources.

QUESTION: Will the TIMP functions associated with monitoring the research efforts of other countries be affected by the user-fee system?

ANSWER: The Department fully intends to make its best efforts to fully recover costs. However, if funding cannot be fully recovered through user fees, in all likelihood a reduction or delay of some activity may be necessary in the acquisition and exchange of foreign information.

QUESTION: How will the implementation of a user-fee system affect the timeliness of providing research results to users? Has the Department received any comments from user agencies regarding the implementation of a user-fee system? If so, please provide a summary of these comments.

ANSWER: As previously mentioned, the Department is still reviewing the implementation of user fees. One of our main concerns is to devise a method which will minimize the administrative burden on the user while assuring prompt processing and dissemination to increase research productivity. DOE users have expressed concern over changes in the method of financing this program.

QUESTION: What will be the impact on the TIMP if the full $6,000,000 is not recovered in FY 1986? Will such a situation necessitate a reduction in funding for the program? What specific activities in the program would be affected? How will such a situation affect the future of the TIMP overall?

ANSWER: We anticipate full cost recovery once a specific option has been selected and implemented. However, if full cost recovery is not possible, certain functions will be delayed or curtailed. Functions which would most likely be affected include: the receipt and control of DOE software, the collection and maintenance of a comprehensive DOE research in progress file, and a cutback in automatic distribution of R&D results to DOE researchers.

MANAGEMENT INITIATIVES

QUESTION: Please provide current information on the implementation status of each of the eleven (11) management initiatives (Administrative Organization Management; Administrative Systems; Administrative Process/Field Structure; Payroll/Personnel; Procurement Reform; Cash Management; Financial Systems Reforms; Printing/Publication; Travel Management; GS/GM 11-15 Bulge Reduction; and Reduced SLUC Payments). Please include the following information:

a) The total "saved" in FY 1984;

b) Estimated "savings" for FY 1985;

c) Estimated "savings" for FY 1986;

d) In what ways do each of the initiatives relate to recommendations contained in the report entitled "President's Private Sector Survey on Cost Control" and the OMB report entitled "Management of the United States Government - 1986." Please refer to specific recommendations in these reports, when possible.

ANSWER: Following is the current status of the eleven management initiatives that DOE addressed in the FY 1986 budget:

o Administrative Organization Management: Policy and guidance on organization and staffing has been emphasized at all levels of management. Further, the Department-wide policy on control of organization changes is being more rigidly applied in the central organization and staffing review process. A system on organization structure reviews has been implemented. Two offices, Savannah River and Richland have been reviewed and other reviews are planned. Staffing levels for FY 1984 and FY 1985 were reduced as a result of an internal reorganization of DOE. Due to the reorganization, accompanying administrative functions were reduced. Nevertheless, additional savings will be pursued through such activities as continuing management reviews on where and how administrative functions are performed and through adjustments in organizations and systems. Actual savings in FY 1984 were $1M/20 FTEs; estimated savings for FY 1985 and FY 1986 are $2.5M/30 FTEs and $5.3M/56 FTEs respectively.

This initiative is related to Grace Commission recommendation DOE-01 which would improve the organization and staffing of DOE.

o Administrative Systems: DOE is currently using a revised ADP utilization and sharing plan to better monitor the utilization of, and enhancement of resource sharing of existing computer resources. Guidelines on ADP internal controls have been recently issued. In process are additional guidelines to strengthen ADP economic analyses, performance management, and microcomputer management. DOE has issued a Departmental Order relative to the acquisition, use, and management or microcomputers. Actual savings in FY 1984 were $2M; estimated savings for FY 1985 and FY 1986 are $4M and $6M respectively.

This initiative is related to Grace Commission recommendation DOE-09 which would improve ADP operations and resources and the OMB Report recommendations under the Federal Information Technology Section.

o Administrative Process/Field Structure: Mission statements for the labs have been rewritten to concentrate activities where areas of expertise exist. The lab appraisal process was improved by aggregating the various appraisals during the planning stage. A number of directives have been eliminated and requirements in others have been consolidated and reduced. This past year, 37 reports were eliminated and the burden hours reduced by almost 170,000 hours. In a pilot test conducted by Albuquerque using value engineering techniques, equipment was modified saving the government about $270,000. Reporting involving socio-economic information was reduced significantly by simplifying the process of certifications and representations. This resulted in an estimated reduction of 92,000 burden hours. Other procurement reductions in reporting in subcontracting and small purchases has reduced burden hours by an estimated 50,000 hours. There were no savings projected for FY 1984; estimated savings for FY 1985 and FY 1986 are $35M/30 FTEs and $75M/30 FTEs respectively.

Grace Commission recommendations DOE-03 on reducing "micromanagement" at the GOCO facilities of DOE and DOE-12 on National Laboratory Management are directly related to this initiative.

o Payroll/Personnel: The Departmental automated PAY/PERS system will be expanded to include all the Operations Offices by the end of FY 1985 and other remaining Departmental organizational elements by the first quarter of CY 1986. All other DOE organizations have already been converted to PAY/PERS. There were no savings projected for FY 1984; estimated savings for FY 1985 and FY 1986 are $0.3M/6 FTEs and $1M/14 FTEs respectively.

This initiative is similiar to the OMB Report recommendation on Personnel/Payroll Systems.

o Procurement Reform: This initiative reflects savings through a number of actions designed to improve the utilization and administration of the procurement system. Savings resulting from IG audit findings, as concurred in by the program or field organization being audited, will be subject to withdrawal from current funding available to the audited organization and/or used as offsets to future year's budgets. Until these savings are realized the reductions will be assigned on an interim basis to Departmental Elements. The tentative plan is to return the savings on a quarterly basis to those organizations that effected the savings, thus repaying those organizations where reductions were taken. The A-76/Productivity Review program is concentrating on commercial activities in 14 work areas and other selected large activities designated by OMB, plus productivity reviews of the operations and maintenance functions at the Power Marketing Administrations. Almost 3,500 FTEs will be reviewed over the next four years. DOE continues to assess the impact of P.L. 98-620, which modified the provisions of P.L. 96-517 pertaining to patent policy for GOCO contractors. Under the law, the Department of Commerce (DOC) has responsibility to develop and issue implementing regulations. DOC has drafted these regulations and DOE has provided comments. We are awaiting DOC reactions. Actual savings in these areas for FY 1984 were $9.3M/50 FTEs; estimated savings for FY 1985 and FY 1986 are $18.5M/1 FTE and $21.1M/1 FTE respectively. The FY 1985 and FY 1986 estimated savings do not include savings for FTEs for the A-76 program . The actual FTE savings will be determined after completion of the A-76 studies.

This initiative is similiar to Grace Commission recommendations DOE-04 on implementation of the OMB Circular on A-76 and DOE-10 on increased audit coverage by the Inspector General. In addition, the initiative is related to the OMB Report on Procurement Reform, A-76 Reviews, and In-House Efficiency Reviews.

o Cash Management: The Department has made improvements in the methods of billing and collecting receipts as well as in the disbursement of funds. Currently, over 99% of payments are made in a timely manner. Actual savings in FY 1984 were $89.1M in budget outlays. Estimated budget outlay savings for FY 1985 and FY 1986 are $31.7M and $36.3M respectively.

This initiative is directly related to the OMB Report recommendation on Cash Management.

o Financial Systems Reforms: DOE initiated new policies and procedures to better capture the recoveries resulting from deobligations of prior year obligations. The funds recovered will be used to offset FY 1986 EWD Appropriation requirements and FY 1987 Interior Appropriation requirements. In order to improve and coordinate the Department's accounting systems the following projects are underway: All payrolls within the Department will be converted to a single integrated Payroll/ Personnel System (PAY/PERS) by the first quarter of CY 1986; accounting systems used by Headquarters and major field offices will be converted to a single Departmental Integrated Standardized Core Accounting System (DISCAS) by the end of CY 1985; and the Departmental Financial Information System (FIS) will incorporate an automated interface to receive, summarize and report newly available financial data from DISCAS. Actual savings for FY 1984 in these areas were $58.8M; estimated savings for FY 1985 and FY 1986 are $39M and $34M respectively.

Grace Commission recommendation DOE-08 on accounting systems in DOE is related to this initiative as is the OMB Report recommendation on Financial Management Systems.

o Printing/Publications: The Albuquerque duplicating shop was shut down in October 1984, and replaced with an on-site, self-service high-speed copying machine. The DOE Headquarters duplicating plant was converted to a contractor operation on September 30, 1984. The BPA print shop cost study determined that it is more economical for DOE to keep the on-site print shop open and operated by DOE personnel. Organizational improvements identified during the BPA review are being implemented. A formal review (similiar to the BPA study) of the Office of Scientific and Technical Information printing facility is underway and results are expected in July 1985. There were no projected savings for FY 1984. Estimated savings for FY 1985 and FY 1986 are $1.1M/57 FTEs and $1.1M/0 FTEs respectively.

This initiative is directly related to Grace Commission recommendation DOE-04 pertaining to increasing utilization of commercial printing facilities and the OMB Report recommendation on Printing.

o Travel Management: In response to this management initiative, the Department has closed (or in the process of closing) the travel offices located in the Albuquerque, Chicago and San Francisco Operations Offices. Extensive analysis has been conducted of the two offices located in the Washington metropolitan area which provide service to the DOE Headquarters employees. This analysis revealed that employees are engaged in a variety of travel functions, a number of which should be performed by Federal employees. Therefore, DOE has decided not to close the Washington area travel offices at this time. There were no projected savings for FY 1984. Estimated savings for FY 1985 and FY 1986 are $0.6M/13 FTEs and $0.6M/0 FTEs respectively.

The OMB Report recommendation on Travel is directly related to this initiative.

o GS/GM 11-15 Bulge Reduction: The Department is continuing to review its grade structure at the GS/GM 11-15 level to determine where further savings can be made. Note that the number of GS/GM 11-15 positions have been reduced by approximately 15 percent since 1981. There were no projected saving for FY 1984. Estimated savings for FY 1985 and FY 1986 are $1.7M and $5.2M respectively.

This initiative is not directly related to any DOE Grace Commission recommendation or OMB Report recommendation.

o Reduce SLUC Payments: Based on the most current data, the DOE utilization rate for all office space is below 135 square feet per person, the GAO goal. Our present goal is to continue to maintain a utilization rate at or below the target set by GSA. There were no projected savings for FY 1984. Estimated savings in FY 1985 and FY 1986 are $4.3M in each year.

This initiative is directly related to the OMB Report recommendation on Space Reduction.

MANAGEMENT INITIATIVES

QUESTION: Please describe any new data which has been obtained or new developments which have arisen on review of the implementation of these initiatives.

ANSWER: The Department carefully monitors the results and implementation of the management initiatives program. There have not been any new developments or data changes that would significantly impact these initiatives. The current estimates for savings of dollars and FTEs are achievable and should be made within the specified time frames.

APPENDIX II

SUPPLEMENTAL QUESTIONS AND ANSWERS

Department of Energy
Washington, DC 20585

May 16, 1985

Honorable Marilyn Lloyd
Chairman, Subcommittee on Energy
  Research and Production
Committee on Science and Technology
House of Representatives
Washington, D.C. 20515

Dear Madam Chairman:

On March 8, 1985, Alvin W. Trivelpiece, Director of the Office of Energy Research, appeared before the Science and Technology Subcommittee on Energy Research and Production to discuss the Fiscal Year 1986 authorization request for the Magnetic Fusion Energy Program.

Following that hearing, you submitted written questions for our response to supplement the record. Enclosed are the answers to those questions, which also have been sent directly to the Committee staff.

If you have any questions, please call Ingrid Nelson or Cathy Hamilton of my staff on 252-4277. They will be happy to assist you.

Sincerely,

Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosure

bcc: Nancy Jeffery

POST-HEARING QUESTIONS AND ANSWERS
RELATING TO THE
MARCH 8, 1985 HEARING
BEFORE THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

WITNESS: DR. ALVIN TRIVELPIECE

Question 1: The budget states, "Studies of experimental machines to ignite fusion fuel and produce a burning plasma will be the primary focus of the systems design studies in FY 1986." Will you please elaborate on the work to be undertaken in the area of copper coil designs as well as those with superconducting coils?

Answer: In FY 1986, a design team will choose specific features of a compact, copper coil machine from among the concepts developed in the FY 1985 studies. The team will begin system design studies of this machine to establish the costs, schedule, needed project R&D, and performance potentials for a compact ignition experiment project. System design work will also continue to establish the most cost effective superconducting machine that ignites, has a burn time sufficient to establish the science of steady-state equilibrium burn, and demonstrates superconducting magnet technology. More than 70 percent of the ignition work will be on the copper machine.

Question 2: What impact will the reduction in the fusion program budget have on the training of graduate students?

Answer: Approximately 18 graduate students will be lost to the fusion program as a result of close-out and reduced funding of Applied Plasma Physics-supported programs and an additional 15 students will be lost as a result of the reduced funding for the Divisions of Development and Technology and Confinement Systems.

Question 3: What would be the impact of shutting down the neutral beam test facility for plasma heating at the Lawrence Berkeley Laboratory, independent of RF heating research, especially in light of the negative ion work at Berkeley?

Answer: The Berkeley test facility addressed by the question is only one of several neutral beam test facilities at the Lawrence Berkeley Laboratory. It is named the Neutral Beam Engineering Test Facility (NBETF). The NBETF is being used to complete development of the long pulse positive ion neutral beam sources for TFTR, Doublet-IIID, and MFTF-B. Development of these sources will complete the positive ion neutral beam development program as it is presently envisioned. The Berkeley negative ion development effort is progressing well, and we would expect it to continue to do so for some time by continued use of the other Berkeley test facilities.

Question 4: What is the nature of the Mini-Mars work that is referred to in the budget? In what respect will the configuration of Mini-Mars be less complicated than current mirror configurations?

Answer: Mini-Mars is a tandem mirror power reactor design of 600 MWe. The present work is to provide a design of this small reactor sufficient to estimate costs, complexity, feasibility, and needed R&D for this configuration. The simplification of Mini-Mars derives from its use of axisymmetric octopole magnet end cells. These end cells are much shorter than the multiple Yin-Yang magnet configurations of the prior MARS (and MFTF-B) design, and, therefore, use less conductor and require less auxiliary heating. The simpler and shorter octopole end cells allow a shorter central cell as well.

Question 5: When the budget refers to a "compact toroid confinement approach," does that mean Ohmic Heating Toroidal Experiment (OHTE)? If so, what will be the Government's role in this program, and will it extend to operating and testing the device at a Government laboratory?

Answer: The "compact toroid confinement approach" refers to a class of toroidal magnetic confinement approaches which includes the field-reversed configuration (FRC) and the spheromak. It does not include the OHTE approach. The OHTE approach is similar to the reversed-field pinch approach (RFP) that the program supports with research principally at Los Alamos National Laboratory. Currently, there is no Government role in the future OHTE program.

Question 6: Would it be correct to state that the fusion program should consider three primary goals consisting of 1) good science, 2) demonstrating sustained plasma ignition and 3) developing an integrated system and industrial base leading to a product. If so, how is the Department's program directed toward attaining these goals?

Answer: The goal of the recently issued program plan for the Magnetic Fusion Energy program is to establish the scientific and technological base required for fusion energy. In order to reach this goal four key issues must be addressed including magnetic confinement systems, properties of burning plasmas, materials for fusion systems and nuclear technology of fusion systems. Three strategic objectives are required to reach the program goal. First, there is a science objective to be able to predict, control and optimize the behavior of plasma confined in fusion-relevant magnetic configurations. The second is a technology objective to develop the unique fusion components and subsystems under conditions relevant to fusion energy sources. Finally, the technology transfer objective is to provide a range of options for private sector investment. The Department's program has been reoriented over the past several years to focus upon these strategic objectives.

Question 7: Has the pace for investigation of generic plasma phenomena associated with a burning plasma slipped relative to the pace at which the other areas of the Magnetic Fusion program are being pursued?

Answer: No, the pace of the entire fusion program has been slowed in response to the budget reductions.

Question 8a: Please present, for the record, a "bar chart" type schedule, showing both the starting and completion dates for each of the following events:

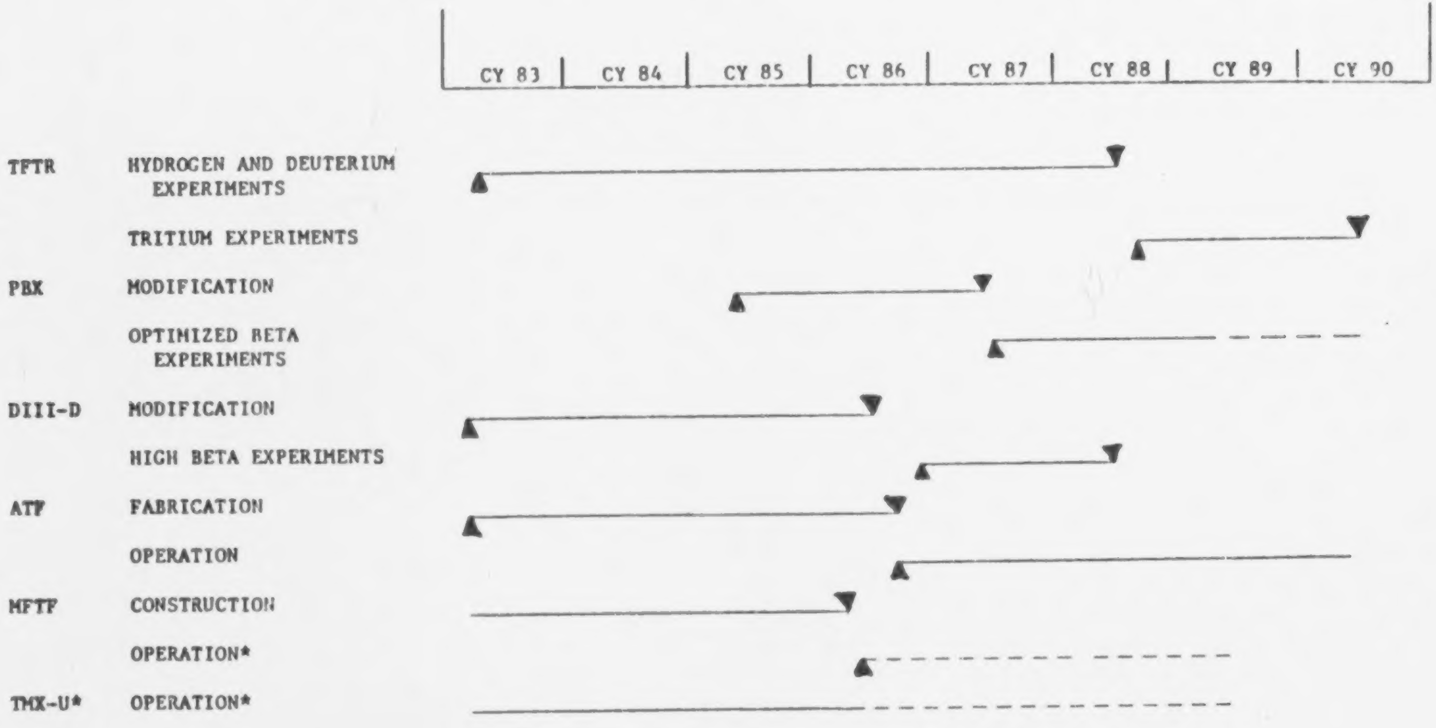
--TFTR plasma experiments using hydrogen
--TFTR plasma experiments using deuterium
--TFTR experiments using tritium
--Modification of the Princeton Beta Experiment (PBX)
--Doublet III-D modifications
--Doublet III-D high beta plasma experiments
--Fabrication of the ATF stellarator
--ATF plasma experiments
--Completion of MFTF-B construction
--TMX-U experiments

The attached chart gives the current plans, which are, of course, subject to the availability of funds.

Question 8b: Please submit, for the record, the projected costs for each of the items listed in the previous question, including all associated operating, PACE, and capital equipment costs.

Answer: Costs

MFTF-B	$364,140,000	
TFTR (H-D)	$455,000,000	
TFTR (Tritium)	$126,000,000	
PBX Modification	$ 10,700,000	
Doublet III-D Modification	$ 35,850,000	
Doublet III High Beta	$ 64,000,000	
TMX-U	$176,000,000	through FY 1989
ATF Experiments	$ 72,000,000	through FY 1990
ATF Fabrication	$ 24,200,000	



* A DECISION AS TO WHEN TO TERMINATE TMX OPERATION AND BEGIN MFTF OPERATION WILL BE MADE AT A LATER DATE.

Question 9: How successful has the work carried out at the University of Wisconsin been and how does it relate to other fusion research?

Answer: Overall, the University of Wisconsin ranks as one of the most effective universities contributing to plasma fusion development. At Wisconsin, fusion research is conducted by individual principal investigators in several different departments and covers almost the full spectrum of areas of fusion science. The quality of research and both the number and quality of students trained for fusion relevant careers have been high. Recent Wisconsin research on mirror, tokamak, stellarator, and reversed-field pinch confinement concepts has been productive and well integrated with major facilities development for advanced tests of these concepts. The plasma theory program at Wisconsin has been a particularly valuable source for evaluations of plasma physics issues. The current reactor systems studies provide sound engineering analysis and design in support of large study projects.

Question 10: What will be the leasing costs associated with the new Cray 2 computer to be installed this month? Was the Cray 1-S also leased? If not, where is it being moved and what compensation, if any, will be paid to the Government? What is the installation cost of the Cray 2, or is that included in the lease agreement?

Answer: The Cray 2 computer will be acquired on a 60-month lease-to-ownership agreement with a monthly payment of $306,000. The Cray 1-S was also acquired via a lease-to-ownership agreement which we plan to complete in October of 1985. The Cray 1-S will remain at the Magnetic Fusion Energy Computer Center. The installation costs for the Cray 2, including site preparation for cooling and electrical load, will total $150,000. These one time installation costs are in addition to the monthly lease costs.

Question 11: What specific contributions were made by the Magnetic Fusion Computer Center in 1984?

Answer: The MFE Computer Center is continuing to provide supercomputer access to scientists in a cost-effective manner which has been used as a model by other programs and agencies. One specific technical accomplishment in 1984 was the conversion of the Cray Time Sharing System to provide interactive access to the Cray X-MP installed in 1984 and the Cray 2 to be installed in 1985. This work generalized the Cray Time Sharing System to appropriately handle a computer with multiple central processor units. The X-MP at the MFE Computer Center has two processors. The Cray 2 will have four. This accomplishment will make users more effective by providing them with the same time sharing system on all of the Cray computers at the MFE Computer Center. Other major accomplishments include improving diagnostics for network functions and loading, and the conversion of many graphics and mathematical libraries to function in computers with multiple central processor units.

Question 12: What progress has been made on the TMX-U during the past year?

Answer: Experiments have demonstrated the existence and the effectiveness of the thermal barrier to confine plasma in discharges with modest central cell densities. In addition, experiments showed that electrically insulating the end walls reduced the radial loss of ions in the central confinement region. Design values for the hot electron energy density in the end plugs were achieved. The capacity for radio frequency heating of TMX-U central cell ions was expanded with the addition of a second RF transmitter, and experiments with 400 kW of transmitted power in two antennas have been carried out. Numerous plasma diagnostics were added during the past year to provide a significantly improved capability for the determination of plasma properties, containment, and transport.

Question 13: How does the TMX-U work tie into the mirror work being done at MIT? How does TARA relate to endplug improvement?

Answer: TARA and TMX-U, which began operating in 1984 and 1982, respectively, are the leading U.S. tandem mirror devices. In many instances, the work is complementary, and, in others, independent investigations are made. In addition to using LLNL magnets on TARA, MIT benefited in designing TARA from previous TMX work. Some areas where there has been a strong contribution to the TARA program from TMX-U include plasma heating, thermal barrier physics, magnet alignment, fueling, gas control, and theoretical analysis.

The emphasis of end cell improvements is to provide greater symmetry to the magnetic fields, which is predicted to give better confinement and lower capital cost reactors. The TARA axicell configuration was designed to be more symmetric than TMX-U. The TARA design provides flexibility which can be utilized to modify TARA to test other end cell improvement approaches as well.

Question 14: What TMX-U work is planned for the remainder of FY 1985 and how is this work being coordinated with MIT and other fusion subprograms?

Answer: The basic plan is to operate the machine as presently configured using new diagnostics to determine what prevents operation at higher central cell densities. Systematic, detailed measurements will be made as various quantities, such as electron cyclotron resonance heating power, ion cyclotron resonance heating power, fueling rates, neutral beam power, etc. are varied for available operating modes. This work is coordinated with other mirror work through DOE-initiated reviews and through quarterly meetings of all tandem mirror experimental groups.

Question 15: What TMX-U experiments are planned for FY 1986, what results are anticipated and how will these contribute to the overall Magnetic Fusion Energy program?

Answer: By FY 1986, TMX-U will be configured to begin experiments that deal explicitly with the important issue of end cell magnetic field symmetry and its impact on radial transport of energy. The high-field throttle coil magnets at each end of the long symmetric solenoid, or central cell, will be installed. Then the magnetic configuration can be varied from the present configuration, with a quadrupole (nonsymmetric) end cell to a configuration with most of the central cell particles isolated from the quadrupole by the high-field throttle coils. The results from these experiments will indicate the level of symmetry required for optimum efficiency in a reactor embodiment. In FY 1986, the hardware to begin testing the use of RF power to pump unwanted ions out of the thermal barrier region will also be installed. As presently configured on TMX-U, this pumping is done with a high power particle beam, and the gas from the neutral beam causes problems with particle control. At this time we expect to use radio frequency pumping and throttle coils on MFTF-B.

Question 16: The budget refers to preparations for limited tritium operations at the TFTR. Will you describe these operations? The budget also refers to attaining energy breakeven conditions in a deuterium plasma at TFTR. What are these conditions?

Answer: The present plans are for TFTR to perform some limited experiments using tritium beginning in early FY 1989. The objective of these experiments will be to achieve breakeven in a deuterium-tritium plasma. "Limited" is used in the sense that the experiments will involve as few plasma discharges as will be necessary to achieve breakeven. This limit is imposed by the need to minimize the total neutron fluence in order to avoid the use of expensive remote handling equipment. An earlier demonstration in deuterium of the plasma conditions required for energy breakeven is planned for the end of calendar year 1986.

There are a range of plasma conditions that will yield energy breakeven. The main plasma quantity that determines energy breakeven conditions is the energy confinement time. If the energy confinement time is greater than .3 seconds, and the ion temperature is greater than 5 keV, energy breakeven conditions will exist in TFTR. With energy confinement times of .3 seconds or greater, the TFTR neutral beams will produce plasma temperatures greater than 15 keV.

Question 17: The budget book seems to indicate that DT plasmas will not be used at TFTR until some time beyond FY 1988. If that is correct, why is it necessary to do tritium preparation work two years ahead of time?

Answer: The current plan is to be ready to initiate tritium experiments in TFTR at the end of calendar year 1988. In order to be ready, a significant amount of preparatory work must still be completed. This preparatory work involves detailed planning, pre-operational component and system testing, and personnel training. This work must be completed in order to provide the necessary level of assurance that the experiments can be performed efficiently and safely. Many systems are involved including tritium supply and delivery, separate cleanup systems for the vacuum vessel, the vault, the neutral beams and the test cell, and heating, ventilation and air conditioning. All of these must be integrated with and controlled by the facility control system. Detailed program planning for each of the tasks has been reviewed recently by a team comprised of experts outside of the Princeton Plasma Physics Laboratory. The team has confirmed the necessity of proceeding with this work in 1986, 1987 and 1988.

Question 18: What hydrogen plasma experiments are planned for TFTR beyond FY 1986?

Answer: The FY 1987-1988 time period on TFTR will be used for experiments on the compression of neutral beam-heated and pellet-fueled plasmas, and for further optimization studies of reactor-relevant plasmas. These optimization studies will be in preparation for the limited D-T experiments.

Question 19: Does DOE have any specific plasma criteria against which newly proposed experiments would be rated before initiating new device fabrication? If so, what is it?

Answer: There is no specific plasma criteria upon which new experiments are rated. Projects are judged on their scientific and technological merit relative to the overall program mission. Recognizing the various concepts and issues addressed by the fusion program, a comparison on the basis of specific plasma criteria would be difficult and impractical.

Question 20: The FY 1986 budget states that nuclear technology is a "key element in determining the safety, economics and impact of fusion." Would you comment on the suitability of the MFTF-B as a facility for nuclear technology development? Specifically, would it be significantly easier, and therefore less costly, to design, build, and test a small lithium blanket in the solenoid section of the MFTF-B than it would be to undertake a similar task in a tokamak? Would the design and fabrication of remote handling equipment also be simplified and less costly? How would such device planning relate to the overall program schedule?

Answer: Nuclear technology development requires experiments in a broad spectrum of facilities including non-neutron test stands and neutron-producing test facilities such as point sources, fission reactors, and fusion devices. Of the neutron-producing facilities, fusion devices are the most expensive. However, ultimately a fusion neutron source will be needed to provide prototypical fusion conditions. Fusion devices based on both tandem mirror and tokamak plasma confinement concepts have been studied. Because of the limited extent of these studies and the large uncertainties involved, it is not yet clear which concept would make the most attractive fusion device for nuclear technology development. Future studies will help clarify this issue as more is learned about key figures of merit such as technical risk, cost, and ability to meet nuclear testing requirements.

Question 21: The budget states, "the scheduled acquisition and installation of auxiliary components (vessel, magnet, cryogenic, vacuum, computer and power supply systems) which are required to conduct an experimental program, will be postponed to reduce the overall fusion program budget." With all this postponement, what is being delivered and when did vessels, magnets, cryogenic and vacuum systems become "auxiliary pieces" of equipment? What is the total cost of all these systems and equipment?

Answer: The MFTF-B auxiliary components that will be postponed include: diagnostics, long pulse neutral beam sources, neutral beam source cables, drift pumping systems, vacuum accessories, injector accessories, gettering systems, and portions of the ECRH heating systems.

All of the basic facility funded under the PACE project, including the vessel, superconducting magnets, cryogenic system, vacuum system, computer control systems, neutral beam power supplies, building modifications, etc., will be completed and a full acceptance test of the facility is planned for the end of calendar year 1985. The cost of the PACE Construction Project which includes the basic MFTF-B facility is $241.6M. The cost of the auxiliary components is approximately $130M.

Question 22: What diagnostic equipment would be needed for the MFTF-B and what is the estimated cost of this equipment? What work force in terms of scientific, engineering, technician and other labor would be needed to operate the MFTF-B and to conduct experiments?

Answer: The diagnostic array for MFTF-B will evolve from simple start-up levels, where minimum plasma parameters are measured to facilitate machine start up, to the full capabilities of the basic diagnostic set, which are capable of discerning detailed spatial and temporal phenomena within the plasma. The start-up set of diagnostics would cost approximately $20M. This set would characterize the essential axial confinement parameters and behavior in the start-up environment, and identify major elements hindering expected machine operation. An abbreviated list is the following: a magnetic field alignment system, an x-ray spectrometer, a diamagnetic loop, an end loss current analyzer, an end loss ion spectrometer, a neutral flux spectrometer, a far infrared interferometer, a microwave interferometer, and RF probes. Additional diagnostics needed for radial measurements and those needed to answer more detailed physics questions will cost an incremental $20-25M in later years. In many cases, the additional diagnostics consist of large arrays of the start-up diagnostics instead of single instruments. In this manner, spatial profiles of parameters are assembled. In other cases, a single highly sophisticated diagnostic is added, such as Thomson scattering, for measurements of spatially resolved electron temperature and density.

The work force required to operate MFTF-B and to conduct experiments would be 210 people.

Question 23: What would be the annual operating costs of MFTF-B? What specific experiments would be conducted? If further work on the MFTF-B was deferred, what other mirror confinement systems work would you continue to pursue?

Answer: The annual operating cost for MFTF-B would be $50M/year in FY 1986 dollars.

The experiments to be run on MFTF-B by year would be the following:

FY 1987	Prepare for operation;
FY 1988	Test stability of configuration with half-second beams only;
FY 1989	Test axial and radial confinement with half-second operation;
FY 1990	Install 30-second beams;
FY 1991	Achieve high $n\tau$ operation.

If further work on MFTF-B were deferred, other end cell configurations that could improve the concept (such as axisymmetric plugs on TARA and octupole end cells on TMX-U) would be explored. These alternate end cells have the potential for making a tandem mirror reactor more maintainable and more efficient.

Question 24: Have any analyses been performed which compare the differences in cost between continuing with the MFTF-B to deferring the project to operation at some later date?

Answer: No detailed analyses of this kind have been performed. While current outlays can be reduced by deferring operations, it is also clear that higher overall costs will be involved with reactivating a standby MFTF-B facility later rather than continuing with the project at this time.

Question 25: What impact would the deferral of MFTF-B have on the Livermore work force and to what extent could this be offset by other programs? What meaningful mirror work would go ahead on other devices at Lawrence Livermore Laboratory?

Answer: Deferral of MFTF-B will reduce the MFTF-B work force from the FY 1985 level of 229 to 80 for FY 1986. Attempts are being made to place as many as possible within other programs within the laboratory, but some reductions-in-force will occur.

TMX-U at LLNL, as well as TARA at MIT, will continue to operate to increase the scientific data base for thermal barrier tandem mirror systems, as well as to explore alternate end cell configurations that could improve the overall mirror concept. Concept improvement includes various symmetric magnetic geometries, alternate approaches for stability, and heating systems other than neutral beams. LLNL will also work on technology improvement in the areas of gas control, wall conditioning, electron and ion cyclotron resonant heating, alternative fueling techniques, and plasma diagnostics.

Question 26: Are two targets now in operation at the Rotating Neutron Source (RTNS)? What experiments have been conducted and what were the results? Have any new alloys been identified for potential use in future fusion devices?

Answer: There are two targets now in operation on RTNS-II. This facility is used for low fluence radiation damage studies using 14 MeV neutrons in metals, alloys, ceramics, and diagnostic components. One of the important discoveries made by U.S. and Japanese researchers in using RTNS-II is that low fluence damage due to 14 MeV neutrons is not fundamentally different from that produced by fission neutrons and that such low fluence damage can be accounted for on the basis of current theories. While these kinds of low fluence studies do not lead directly to the development of new materials or alloys which are resistant to the high damage levels of reactors, they do provide a foundation on which to build alloy development research. Alloy development research utilizing existing fission test reactors and ion irradiation has yielded significant advances in ferritic alloy steels, in austenitic stainless steels, and in vanadium alloys.

Question 27: Has any radiation damage testing been performed on superconducting materials through an "in-pile" test in an operating reactor? Would any testing along these general lines make sense?

Answer: Radiation damage testing on superconducting materials is taking place both in fission materials testing facilities and in the RTNS-II. Much fission reactor testing has been funded by the Office of Basic Energy Sciences of the DOE. This data forms a solid base to which fusion testing in RTNS-II has been complementary. For fusion, no additional fission reactor testing on superconducting materials is necessary until new fabrication methods and configurations have been chosen for particular applications.

Question 28: Please give a summary of all materials R&D included within the fusion program, including description of work, responsible organizations, and funding levels for FY 1985 and FY 1986.

Answer: The materials research program is managed by the Reactor Technology Branch in the Division of Development and Technology. It is based on the two major technical issues that are specific to fusion--plasma interactive materials and neutron radiation interactive materials. These are organized into eight sub-elements with funding breakdown as follows:

	($ X 10^6)	
Plasma Interactive	FY85	FY86
High Heat Flux Components	1.43	1.40
Plasma Interactive Materials	3.31	3.70
Plasma Interactive Systems	0.40	0.0
Neutron Radiation Interactive		
Alloy Development for Irradiation Performance	4.27	3.95
Special Purpose Materials	1.10	1.30
Damage Analysis and Fundamental Studies	1.99	2.40
Analysis and Evaluation	0.15	0.15
Radiation Facilities Development and Operation	3.90	3.30
	16.55	16.20

These sub-elements of the program are each guided by technical level task groups drawn from laboratory, university, and industrial participants. National laboratories participating include ANL, Sandia, ORNL, HEDL, LLNL, LANL, and PNL. University participants include Auburn University, MIT, UCLA, UC Santa Barbara, and the University of Wisconsin. Industrial contractors include GA Technologies, Inc., Rockwell International, and McDonnell Douglas.

Question 29: Is the Rotating Neutron Source (RTNS) work at the Lawerence Livermore National Laboratory sufficient in scope and facilities to provide the experimental data essential to meeting program objectives? Is there any device beyond RTNS, but one that would be less costly than FMIT, which could more closely simulate the fusion environment?

Answer: The Rotating Target Neutron Source (RTNS-II), a low fluence, small volume, mono-energy, 14 MeV neutron source, is not sufficient to provide the necessary high fluence, large volume, end-of-life experimental data required to meet current or future materials program objectives. A comprehensive review by an IEA Blue Ribbon Panel (chaired by Sir Alan Cattrell) in 1983 identified no viable alternative and strongly endorsed the FMIT. We now are pursuing an initiative within the IEA to define an international fusion materials program, addressing all aspects of materials research and development including the need and use of test facilities.

Question 30: How is other on-going materials research that is being performed under other government programs being incorporated in the fusion program?

Answer: The fusion materials program draws directly on materials research developments of the Office of Basic Energy Science and the advanced fission nuclear reactor program of the Department, especially in the study of radiation effects. In addition, our interagency agreements with the National Bureau of Standards and with the Naval Research Laboratory draw on the expertise of those institutions. Finally, we participate in the Energy Materials Coordinating Committee of DOE for the purpose of general program coordination within DOE. Special topics are further covered by staff and by laboratory personnel on an individual basis.

Question 31: If the U.S. was to cooperate with other nations on the fabrication and operation of an ambitious ignition device ($1.5 billion or more in cost), what would be the major parameters of such a device, that is, what would be its primary features or components? Specifically, would it have a lithium blanket, remote maintenance, and superconducting coils? What would be the advantage of building a large ignition device in this manner versus a lower-cost device assuming that the lower-cost device could be built at least five years sooner?

Answer: An ambitious ignition device would be a long pulse equilibrium burn physics device which includes the nuclear engineering development features such as breeding blankets. The European and Japanese programs want to proceed in the early 1990's with a large scale engineering development facility which would have superconducting coils, full remote maintenance and a lithium blanket.

In a cooperative project with the design described above the blanket design would be a subject of negotiation with the partners involved and would probably consist of modules of several different blanket concepts. The major parameters of the machine including size, field strength, current, neutron fluence, heating technique, would strongly affect total cost of the facility and would be negotiated with the partners based on a joint design activity. The primary advantage of such a machine would be that it would provide a facility to obtain information on two program goals: physics studies of ignition and burning plasma physics in a first phase and nuclear technology test data in a second phase. The primary disadvantages are the higher initial construction cost (probably in the $3B range) and the risk of poor technical performance in the second phase.

A simple long pulse equilibrium device would focus only on ignition physics and could have either superconducting coils or copper coils. It would have full remote maintenance, but it would not have blanket modules. The cost for the long pulse equilibrium burn devices that the U.S. studied during FY 84 was in the $1B to $1.5B range. However, the U.S. is now studying the feasibility of proceeding with a less costly compact copper ignition device. This kind of device would have a shorter pulse (a few seconds), but it would still provide a great deal of physics information on the behavior of ignited plasmas. The U.S. program strategy is to obtain the fundamental physics information on ignited plasmas before proceeding with a major engineering test reactor involving breeding blankets. It may be possible to build a low cost compact copper ignition device in the late 1980's and obtain ignition physics experimental data in the early 1990's to support a cooperative engineering test reactor project by the turn of the century.

Question 32: Isn't there a risk in building an ambitious ignition device before the dynamics of an ignited plasma are understood?

Answer: Yes. The risk is that performance might not meet expectations, or that the machine would be "over designed"; especially, larger in size and with unnecessary auxiliary heating power, to assure satisfactory performance. Over design of the machine would increase its cost.

Question 33: Assuming that a successful lower-cost ignition device is built and operated, then, what would be the next major step on the path towards a peaceful application of fusion energy? How might the next major step differ if a higher cost ($1.5 billion) ignition device were built five years later assuming there had been no prior experience with a lower-cost device?

Answer: The present strategy for developing fusion is to emphasize the development of the scientific and technological components or subsystems of the integrated fusion system, to the maximum extent that is technically feasible. The next few years will be spent defining exactly what facilities will be required for each of four key issues: identifying an attractive confinement concept, studying the properties of burning plasmas, developing fusion materials, and developing fusion nuclear technology. An ignition experiment has been identified as the next major step in the fusion program since this is an essential scientific "component" of fusion. The nuclear technology issue will ultimately require an integrated engineering test facility. However, the scope and the timing of such a facility and its phasing relative to facilities needed to resolve the other major issues has not yet been established. It is unlikely that one facility can resolve all four issues. Progress toward practical fusion power after ignition will probably involve several specialized facilities.

Question 34: What is the nature of the international collaboration program in the stellarator area with Japan and West Germany? What is involved, who is doing what R&D, what devices or other facilities are included and how successful has the program been to date?

Answer: In the case of Japan, the collaboration is carried out through the existing U.S./Japan bilateral fusion cooperation under the auspices of the U.S./Japan Heliotron/Stellarator Joint Planning Committee; two joint workshops and a number of long term scientific exchanges are carried out each year. The collaboration with West Germany has been effected through personnel exchanges carried out on a lab-to-lab basis. An IEA agreement which is expected to be signed this summer by the U.S. and West Germany (and later probably by Japan) will make higher levels of collaboration possible.

The West German research is presently carried out on the Wendelstein VII stellarator soon to be upgraded to the Wendelstein VII-AS. The Japanese research is carried out on the Heliotron-E device at the University of Kyoto. In the U.S. the ATF stellarator at ORNL is expected to begin plasma operations in 1987.

Because these existing experiments operate on different, but complementary, stabilizing principles, rapid progress has been made in stellarator research during the past few years. The collaboration and communication among the three major stellarator groups has contributed significantly to this progress.

Question 35: On an international program such as this, how do we assure that industry benefits are derived by both sides?

Answer: In general, fusion concepts such as the stellarator are in a scientific research phase so that the concept itself is not likely to have commercial applications in the near future. The Office of Fusion Energy does try to provide the opportunities for industry to participate in our international programs through research and hardware development, where appropriate, and through participation in personnel exchanges when their technical experience and skills are relevant. A good example of industrial involvement in an international fusion program is the International Fusion Superconducting Magnet Test Program. Three U.S. industrial firms, Westinghouse, General Dynamics and General Electric, and three foreign firms, Brown-Bovari for Switzerland, Hitachi for Japan, and Seimens for the European Community (EC) have each provided one superconducting magnet for testing. All design information is available to the industrial participants along with licensing provisions that permit one of the industrial participants to produce another's design. All the test data and analyses will be shared among the participants.

As part of cooperative activities there always exists the potential for the invention by individuals of specific hardware or components that may have more immediate commercial applications. Therefore, provisions are written into our international agreements that provide access to the US government as well as the other national governments to all technical information produced in the tasks of these agreements. This information in turn is available to US industry. In situations where patentable inventions could be produced as a result of work done under the terms of the agreement, DOE will, in general, hold title to the invention in this country. DOE could then license the invention to US industry. In other countries US industry would receive a non-exclusive, irrevocable, royalty-free license. The exact patent terms depend on the type of agreements and which nation supports the inventor. The agreements also protect industry interests by protecting proprietary contributions to joint work.

Question 36: Is the D-T "scientific feasibility" experiment at the Princeton TFTR on the critical path for subsequent ignition experiments? Specifically, does the slippage in this program imply a corresponding delay in the time at which the DOE will have achieved the capability to execute an ignition experiment?

Answer: No. Only the D-T aspects of the "scientific feasibility" experiments on TFTR have been deferred. Demonstration of Q equals 1 equivalent plasmas in deuterium will take place at the end of 1986 as originally planned and will provide a solid experimental base for proceeding with an ignition experiment on an expeditious schedule. Subsequent D-T operation of TFTR will demonstrate key aspects of tritium technology on a tokamak prior to operation of an ignition device and also provide the first experiments with alpha particle confinement.

Question 37: How successful has the international collaboration program been with West Germany, in the area of ash and impurity removal, and how does that work relate to R&D at Princeton?

Answer: The international collaboration program with West Germany involves U.S. designed pumped limiter experiments on the TEXTOR tokamak at Juelich and divertor experiments on the ASDEX tokamaks at Garching. Collaborations with both of these laboratories have been very successful. At Juelich, a U.S. team has been conducting pumped limiter experiments on TEXTOR jointly with the German scientists. Furthermore, the laboratory has agreed to modify the TEXTOR tokamak to conduct the next phase of the pumped limiter experiment, proposed and designed by the U.S. team and funded jointly by the U.S., EURATOM, and Japan.

The ASDEX is similar to the Princeton Poloidal Divertor Experiment, which has been converted to the Princeton Beta Experiment. Several U.S. scientists are already participating in the ASDEX experiments and they will participate also in the future ASDEX-Upgrade experiments.

PPPL scientists participate in exchanges with both German laboratories and utilize experimental data from these devices to verify and improve computer models used to interpret data and design impurity control systems for new devices. Thus, this collaboration is providing the U.S. program a cost effective way to obtain the necessary database on pumped limiters and poloidal divertors.

Question 38: What has been the Japanese reaction to the termination of FMIT activity in the U.S.? Aren't the diplomatic impacts of SRC-II and FMIT terminations much more serious for our partners than they would have been if the U.S. were the affected partner?

Answer: There has been no official Japanese reaction to our original proposal for three party FMIT collaboration or our recent actions. While the management of the Japanese fusion program has continued to express strong technical endorsement and management willingness to consider collaboration on FMIT construction, no formal ministerial level or governmental positions have been expressed. Although the Japanese are probably disappointed at the FMIT termination, there appears to be no diplomatic impacts.

Question 39: What impact will the $600,000 reduction in the Radiation Facilities program have on the Japanese government in FY 1986 and beyond? Will it erode our credibility in future agreements further if we reduce our effort in this one?

Answer: The FY86 reduction of $600,000 in the RTNS-II budget should have no impact on the ability of Japanese government to continue its present and projected studies in the RTNS-II for the remainder of the term of the RTNS-II agreement. The funding reduction reflects the fact that the U.S. program will complete the necessary projected U.S. research in RTNS-II ahead of schedule and does not impede the Japanese program from completing its own projected research.

Question 40: Recognizing that there are certain incentives to perform various tasks in the fusion program in cooperation with other nations, why was it not possible to obtain any agreements for further international cost-sharing on the Fusion Materials Test Facility?

Answer: The DOE initiative was a proposal to establish a three party collaboration for the construction and use of the FMIT on an international basis, shared equally among the EC, Japan and the US. Japanese technical representatives expressed willingness to pursue collaboration in order to obtain materials data. The European community was reluctant to consider the proposed construction collaboration, because the Europeans had not participated in project definitions. It is now clear that there was no common sense of program priority for a materials testing facility nor a consensus on its scope among the potential partners when the U.S. first suggested collaboration on the project.

To ensure a fruitful basis for our collaborative activities in the future, we are emphasizing early joint planning on all topics for which there may be a need for a major international fusion facility. In regard to neutron irradiation testing, in particular, we are pursuing initiatives within the IEA to design an international fusion materials R&D program. Such consideration would involve all aspects of the materials program including testing facility needs and may lay a sound base for future efforts at collaboration.

Question 41: What would the design parameters of the Ohmic Heating Toroidal Experiment (OHTE) have to be to sustain ignition?

Answer: The universal plasma parameters that are needed to sustain ignition are included in the confinement conditions: $n\tau_E \approx 2 \times 10^{14}cm^{-3}sec$ in a thermalized deuterium-tritium plasma at ion temperatures of 7-10 keV. Different magnetic confinement approaches are based on using various means to achieve these conditions. In the case of OHTE, ohmic heating at 7 megaamperes of plasma current is proposed by the developers to achieve the required ion temperatures. At densities of $2 \times 10^{14}cm^{-3}$, energy confinement of about 1 second would be needed. Since the proposed OHTE experiment would use hydrogen gas as a fuel, actual ignition could not be achieved or sustained in any of the proposed near-term experiments. There would be no alpha-particle production to provide the energy necessary to support ignition.

Question 42: Is this device based on the principle of replacing the vacuum chamber after operation, and if so, what is the predicted useful life of a vacuum chamber?

Answer: The devices that are proposed as part of the immediate OHTE program would not be subjected to intense neutron exposure that would be characteristic of devices used in power producing reactors. Therefore, the predicted useful life of a vacuum chamber is unlimited. The immediate OHTE experimental devices might need replacement, but only as part of a research program dedicated to resolving development issues. The predicted useful life of the vacuum chamber in an OHTE reactor is not yet known.

Question 43: How would you describe the differences between OHTE and the Advanced Toroidal Facility?

Answer: The differences between the OHTE approach and the Advanced Toroidal Facility (ATF) are many. While helical current conductors are included in each, their function differs. The primary confinement magnetic fields in the ATF are provided by external windings; plasma currents are unnecessary for plasma confinement. In OHTE the primary confinement magnetic fields are produced by current carried within the plasma. The toroidal magnetic field component is produced by external field coils, as in the tokamak approach. The helical coil in OHTE is used as an auxiliary coil to produce modifications to the primary current generated magnetic field. These modifications could be beneficial; however, an assessment of the future benefit is a goal of proposed research.

Heating of the plasma is also achieved differently in OHTE and ATF. The relatively large plasma current in OHTE is adequate to ohmically heat the plasma. ATF plasma is heated by a combination of auxiliary heating, including neutral beam and rf power.

Question 44: While the existing Advanced Toroidal Facility (ATF) at Oak Ridge is not intended for and cannot be used to achieve ignition, what is your guess as to the future possibilities of using a second-generation ATF to achieve low-cost ignition in the early 1990's?

Answer: The possibility of achieving a low-cost ignition device, via the ATF confinement approach is not considered likely, primarily because the size (and hence cost) of an ignited device using this approach is thought to be significantly larger than the size of the low-cost, compact ignition devices presently being studied.

Question 45: Has there been any computer modeling carried out at the computer center at Livermore on the Advanced Toroidal Facility, and if so, what was the nature of the modeling, what were the results and what conclusions can be drawn from this data?

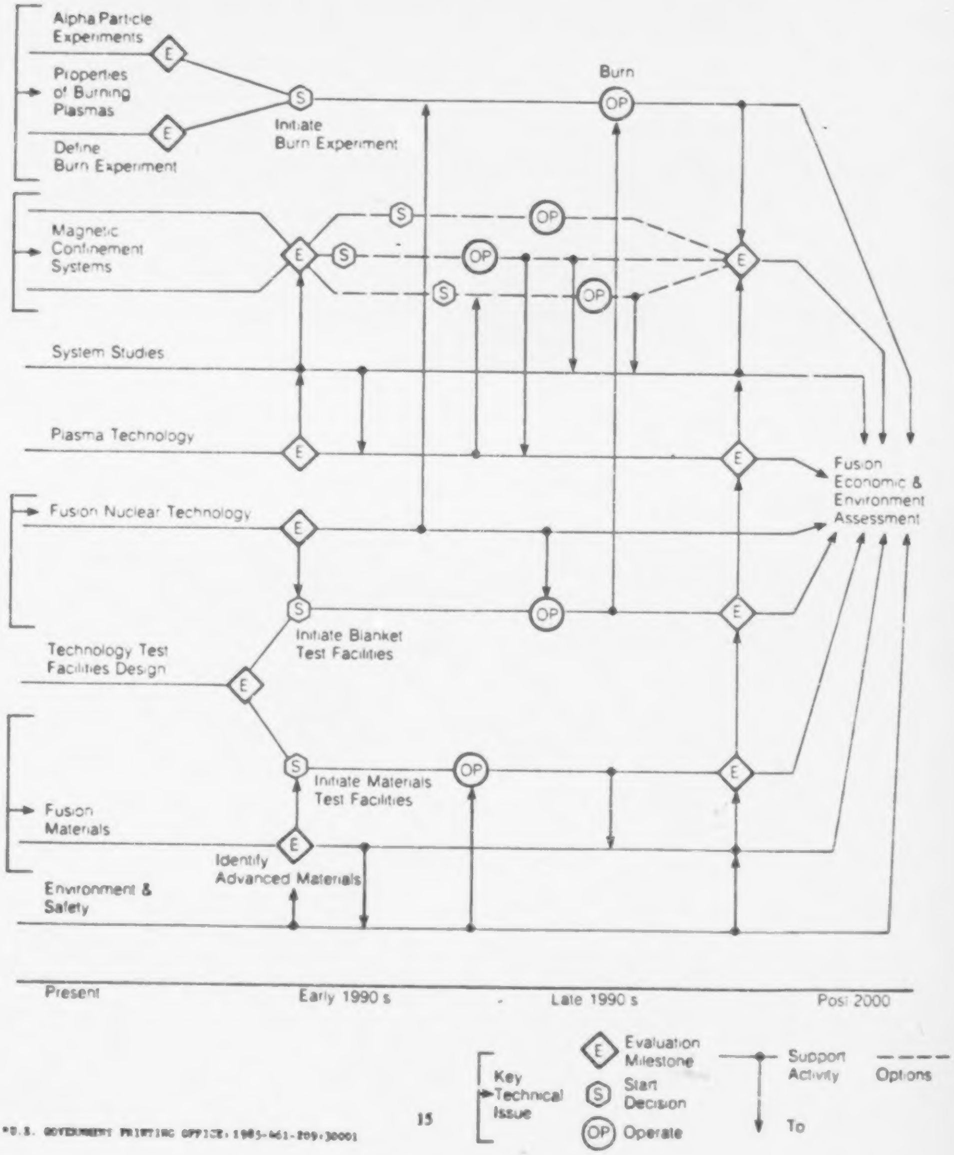
Answer: The CRAY computers at the Livermore NMFECC have been, and are being used for a number of long and complex modeling calculations requiring massive computing power in the physics and engineering design and optimization of ATF. These include vacuum magnetic field line calculations, calculations to assess the confinement properties of different configurations, to design the vacuum vessel shape, and to optimize the neutral beam injection path; equilibrium calculations to optimize the vertical field coil system and to determine the optimum locations for magnetic diagnostics have been done. Calculations to determine beta limits led to the discovery of the potential for a second regime of stability in ATF.

Iterative calculations of several configurations in all of these areas led to the selection of the present ATF design which is now being fabricated. Calculations are in process now to study start-up scenarios, to find the best operating regime for ATF, and to benchmark the model against present stellarator experimental results. Experimental results from ATF will lead to verification and optimization of these codes for design of future devices with a higher level of confidence. In addition to these, ORNL developed a computer model as a part of the ATF program for engineering and machining of complex, three-dimensional shapes with potential industrial applications.

Question 46: What fundamental "scientific" milestones, whose achievements are essential to the development of fusion energy, were accomplished in 1984? That is quantitatively, what milestones have been achieved, what remains to be accomplished and what are the specific priorities for FY 1986?

Answer: The achievements of the program in FY 1984 are listed in my testimony. Specific emphases in FY 1986 include: operating TFTR with full neutral beam power, initial operation of D-III-D, conducting ICRF experiments on Alcator C, all in support of an ignition experiment; modifying PBX, completing ATF, operating S-1, and initiating a new RFP experiment, all in support of improving our available reactor concepts. Major milestones and decisions for the future are summarized in the attached figure from the February 1985 Magnetic Fusion Program Plan.

Magnetic Fusion Program Plan Major Technical Milestones and Decisions


Alpha Particle Experiments
Properties of Burning Plasmas
Define Burn Experiment
E
S
Initiate Burn Experiment
Burn
OP
Magnetic Confinement Systems
S
OP
System Studies
Plasma Technology
Fusion Nuclear Technology
Fusion Economic & Environment Assessment
Initiate Blanket Test Facilities
Technology Test Facilities Design
Initiate Materials Test Facilities
Fusion Materials
Identify Advanced Materials
Environment & Safety
Present
Early 1990 s
Late 1990 s
Post 2000
Key Technical Issue
Evaluation Milestone
Start Decision
Operate
Support Activity
To
Options

Question 47: Would you comment on the potential for such toroidal devices as the reversed field pinch, stellarator, and the compact toroid for overtaking the tokamak and becoming attractive as possible fusion reactors?

Answer: The national fusion development program has not been organized as a competition among the various magnetic confinement configurations. Each approach is supported in order to develop unique information that will add to the overall understanding of confinement physics and the fusion data base. A broad technical base is required to identify an attractive reactor concept for commercial development. Although it is correct that the tokamak approach has achieved performance levels that are superior to the other approaches, we expect that other approaches can be developed faster because of the tokamaks science and technology base.

At present, each confinement approach under development is believed to have unique features which could lead to an attractive reactor. Identification of an optimum magnetic confinement system for a cost competitive fusion reactor is one of the four key technical issues facing fusion.

Question 48: What advancements were made in RF heating in 1984?

Answer: New power handling hardware developed and installed on PLT during 1983 enabled approximately 4 MW of ICRF power to be delivered to the tokamak in 1984. This resulted in plasma temperatures of up to 5 keV, a record for RF heating of a tokamak and comparable to the results obtained to date with neutral beam injection. The key results from this experiment were the successful demonstration of high power RF heating with minimal impurity effects and the fact that energy confinement was equal to or better than that with neutral beams at comparable power levels.

The first high power, high frequency ECH experiments to be conducted outside the USSR were conducted in Doublet III using a cw gyrotron tube newly developed by the fusion program. 1.2 MW of power was delivered to the machine. Good heating was observed, even at densities significantly in excess of the theoretically predicted density limit, indicating the possibility that ECH could be used for heating reactor-grade plasmas.

Question 49: Please give us a comparison between the ZT-H experiment and the Ohmic Heating Toroidal Experiemnt, identifying the similarities and differences between the two.

Answer: The OHTE approach and the reversed field pinch approach (RFP) to magnetic confinement are very similar in nature. The principle difference is the addition of a helical magnetic field coil in the OHTE approach that is used to produce a helical magnetic field concentrated near the edge of the plasma. This field increases the shear (or rotation of the magnetic field direction) and is thought to be beneficial to plasma stability. It is produced at the expense of the simplicity of the basic RFP approach.

Present experiments (OHTE I and the RFP ZT-40M) are similar in size and capability, and have achieved similar performance.

The proposed experiments, ZT-H and OHTE II, differ significantly. The ZT-H RFP experiment, as proposed, is a device with a 4MA plasma current capability that is believed adequate to heat the plasma to 4 keV ion temperature with plasma confinement parameters, $n\tau_E = 2 \times 10^{13} cm^{-3} sec$. OHTE II is proposed to achieve 7MA of plasma current which, if successful, would produce plasma conditions of 7 keV ion temperature and plasma confinement of $n\tau_E \approx 2 \times 10^{14} cm^{-3} sec$. This is considered equivalent to conditions necessary for ignition in a deuterium-tritium plasma. Neither ZT-H nor the OHTE II would use deuterium-tritium as a fuel.

Question 50: The budget refers to new pellet fuel injectors. What is the current state-of-the-art of these injectors and is there any significant research and development work that needs to be done in this area? If so, who is doing it?

Answer: Two types of pellet injectors based on pneumatic principles and centrifugal principles have been developed and used on fusion experimental devices. Pellets as big as 4 mm in diameter have been accelerated to velocities in excess of 1900 m/s and as many as four pellets have been sequentially injected using a pneumatic device. This is a world record. Most major U.S. tokamaks such as PLT, PDX, TFTR, ALCATOR, and ISX have successfully used this type of an injector. Pellets 2 mm in size and at a rate of 20 per second have been accelerated to velocities close to 1000 m/s by the centrifugal device. This type of an injector can be operated continuously for extended periods of time. A centrifugal injector was successfully used on D-III. The development of both types of pellet injectors was carried out by the Oak Ridge National Laboratory (ORNL) which has also been the main supplier of the devices to the various fusion laboratories.

When fusion devices operate at higher plasma temperatures, it is anticipated that pellet velocities in excess of 2 km/s will be required. Preliminary work to improve the performance of a pneumatic device by incorporating an electric arc has been started by ORNL. Research on application of railgun principles to accelerate pellets to velocities in excess of 5 km/s is now being carried out at the University of Illinois.

Question 51: It is conceivable that there will always be new and exciting ideas to harness fusion energy, yet at some point, it will be necessary to select a configuration for a test reactor. Please describe the criteria for making a selection and how such a program can be accomplished in an orderly manner.

Answer: The Magnetic Fusion Program Plan notes that the features which will justify commercial development of fusion depend on the resolution of uncertainties in other energy sources which would be available after the turn of the century. In addition, the characteristics of specific fusion reactors will also be uncertain until current research and development is further advanced. Because of these uncertainties, the program is aimed at satisfying general criteria of economic, environmental, and societal attractiveness. As the time approaches when fusion applications could be considered, more specific criteria for test reactors and commercial energy sources will emerge.

Question 52: What is your evaluation of the Fusion Research Center concept offered by Mr. Mike McCormack during his testimony on March 8, 1985?

Answer: We have no evidence of sufficient industrial interest to justify a Fusion Research Center as described by Mr. McCormack's testimony.

Question 53: Please identify the minimum value of each of the following parameters, or combination of parameters, which is necessary to achieve ignition.

Particle Energy Density
Temperature
Stability Factor
Current Density
Confinement Time

Answer: The minimum temperature at which a plasma will ignite is about 5 keV, and the minimum product of density and confinement time must exceed $2 \times 10^{14} cm^{-3}$ sec. The relevant combination of parameters that determines if a D-T plasma ignites is that the product of plasma density, confinement time, and temperature must be greater than 4×10^{15} in units of cm^{-3} keV seconds. In addition, the other parameters listed are not directly involved in achieving ignition.

Question 54: Please identify the values of each of the above already achieved on the following devices:

T-3
D-III
PLT
Alcator C
JET

Answer: The relevant combination of the parameters that determines if a D-T plasma ignites is the product of plasma density, confinement time, and temperature. This product must be greater than 4×10^{15} in units of cm^{-3}-Kev-seconds. The following is a table of the product of density (N), confinement time (τ) and temperature (T) for the above requested machines. Also included in the table is the current density.

Tokamak	$n \tau T$	Current Density
T-3	2×10^{11}	1.5 MA/m^2
D-III	4×10^{13}	1.3 MA/m^2
PLT	4×10^{12}	1.2 MA/m^2
Alcator C	7×10^{13}	12.4 MA/m^2
JET	1×10^{14}	.48 MA/m^2

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COMMITTEE ON SCIENCE AND TECHNOLOGY
SUITE 2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515
(202) 225-6371

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Republican Staff Director

April 19, 1985

Dr. Alvin W. Trivelpiece
Director, Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Trivelpiece:

Thank you for your excellent testimony at our March 8, 1985 Subcommittee hearing on the FY 1986 DOE Authorization Request for the Magnetic Fusion Energy Program.

We would like to supplement the record of the hearing with your responses to the attached questions. We would appreciate receiving your responses within three weeks from the date of this letter. Please forward this information to the Subcommittee office, B-374 Rayburn House Office Building, Washington, D.C. 20515, to the attention of Ms. Debbie Johnson.

I appreciate your cooperation in this matter.

Sincerely,

Marilyn Lloyd

MARILYN LLOYD, Chairman
Subcommittee on Energy
Research and Production

ML:dj
Attachment

Additional Questions for Dr. Alvin Trivelpiece

- The budget states, "Studies of experimental machines to ignite fusion fuel and produce a burning plasma will be the primary focus of the systems design studies in FY 1986." Will you please elaborate on the work to be undertaken in the area of copper coil designs as well as those with superconducting coils?

- What impact will the reduction in the fusion program budget have on the training of graduate students?

- What would be the impact of shutting down the neutral beam test facility for plasma heating at the Lawrence Berkeley Laboratory, independent of RF heating research, especially in light of the negative ion work at Berkeley?

- What is the nature of the Mini-Mars work that is referred to in the budget? In what respect will the configuration of Mini-Mars be less complicated than current mirror configurations?

- When the budget refers to a "compact toroid confinement approach," does that mean Ohmic Heating Toroidal Experiment (OHTE)? If so, what will be the government's role in this program, and will it extend to operating and testing the device at a government laboratory?

- Would it be correct to state that the fusion program should consider three primary goals consisting of 1) good science, 2) demonstrating sustained plasma ignition, and 3) developing an integrated system and industrial base leading to a product? If so, how is the Department's program directed toward attaining these goals?

- Has the pace for investigation of generic plasma phenomena associated with a burning plasma slipped relative to the pace at which the other areas of the Magnetic Fusion program are being pursued?

  If yes, is this a result of a re-evaluation of the appropriate timing and/or potential benefits to the program, or of unforeseen technical difficulties?

- Please present, for the record, a "bar chart" type schedule, showing both the starting and completion dates for each of the following events:

  --TFTR plasma experiments using hydrogen
  --TFTR plasma experiments using deuterium
  --TFTR experiments using tritium
  --Modification of the Princeton Beta Experiment (PBX)
  --Doublet 3-D modifications
  --Doublet 3-D high beta plasma experiments
  --Fabrication of the ATF stellarator
  --ATF plasma experiments
  --Completion of MFTF-B construction
  --TMXU experiments.

  Please submit, for the record, the projected costs for each of the items listed in the previous question, including all associated operating, PACE, and capital equipment costs.

- How successful has the work carried out at the University of Wisconsin been and how does it relate to other fusion research?

- What will be the leasing costs associated with the new Cray 2 computer to be installed this month? Was the Cray 1-S also leased? If not, where is it being moved and what compensation, if any, will be paid to the government? What is the installation cost of the Cray 2, or is that included in the lease agreement?

- What specific contributions were made by the Magnetic Fusion Computer Center in 1984?

- What progress has been made on the TMXU during the past year?

- How does the TMXU work tie into the the mirror work being done at MIT? How does TARA relate to endplug improvement?

- What TMXU work is planned for the remainder of FY 1985 and how is this work being coordinated with MIT and other fusion subprograms?

- What TMXU experiments are planned for FY 1986, what results are anticipated and how will these contribute to the overall Magnetic Fusion Energy program?

- The budget refers to preparations for limited tritium operations at the TFTR. Will you describe these operations? The budget also refers to attaining energy breakeven conditions in a deuterium plasma at TFTR. What are these conditions?

- The budget book seems to indicate that DT plasmas will not be used at TFTR until some time beyond FY 1988. If that is correct, why is it necessary to do tritium preparation work two years ahead of time?

- What hydrogen plasma experiments are planned for TFTR beyond FY 1986?

- Does DOE have any specific plasma criteria against which newly proposed experiments would be rated before initiating new device fabrication? If so, what is it?

- The FY 1986 budget states that nuclear technology is a "key element in determining the safety, economics and impact of fusion." Would you comment on the suitability of the MFTF-B as a facility for nuclear technology development? Specifically, would it be significantly easier, and therefore less costly, to design, build, and test a small lithium blanket in the solenoid section of the MFTF-B than it would be to undertake a similar task in a tokamak? Would the design and fabrication of remote handling equipment also be simplified and less costly? How would such device planning relate to the overall program schedule?

- The budget states, "the scheduled acquisition and installation of auxiliary components (vessel, magnet, cryogenic, vacuum, computer and power supply systems), which are required to conduct an experimental program, will be postponed to reduce the overall fusion program budget."

With all this postponement, what is being delivered and when did vessels, magnets, cryogenic and vacuum systems become "auxiliary pieces" of equipment? What is the total cost of all these systems and equipment?

- What diagnostic equipment would be needed for the MFTF-B and what is the estimated cost of this equipment? What work force in terms of scientific, engineering, technician and other labor would be needed to operate the MFTF-B and to conduct experiments?

- What would be the annual operating costs of MFTF-B? What specific experiments would be conducted? If further work on the MFTF-B was deferred, what other mirror confinement systems work would you continue to pursue?

- Have any analyses been performed which compare the differences in cost between continuing with the MFTF-B to deferring the project to operation at some later date?

- What impact would the deferral of MFTF-B have on the Livermore work force and to what extent could this be offset by other programs? What meaningful mirror work would go ahead on other devices at Lawrence Livermore Laboratory?

- Are two targets now in operation at the Rotating Neutron Source (RTNS)? What experiments have been conducted and what were the results? Have any new alloys been identified for potential use in future fusion devices?

- Has any radiation damage testing been performed on superconducting materials through an "in-pile" test in an operating reactor? Would any testing along these general lines make sense?

- Please give a summary of all materials R&D included within the fusion program, including description of work, responsible organizations, and funding levels for FY 1985 and FY 1986.

- Is the Rotating Neutron Source (RTNS) work at the Lawrence Livermore National Laboratory sufficient in scope and facilities to provide the experimental data essential to meeting program objectives? Is there any device beyond RTNS, but one that would be less costly than FMIT, which could more closely simulate the fusion environment?

- How is other on-going materials research that is being performed under other government programs being incorporated in the fusion program?

- If the U.S. was to cooperate with other nations on the fabrication and operation of an ambitious ignition device ($1.5 billion or more in cost), what would be the major parameters of such a device, that is, what would be its primary features or components? Specifically, would it have a lithium blanket, remote maintenance, and superconducting coils? What would be the advantage of building a large ignition device in this manner versus a lower-cost device assuming that the lower-cost device could be built at least five years sooner?

- Isn't there a risk in building an ambitious ignition device before the dynamics of an ignited plasma are understood?

- Assuming that a successful lower-cost ignition device is built and operated, then, what would be the next major step on the path towards a peaceful application of fusion energy? How might the next major step differ if a higher cost ($1.5 billion) ignition device were built five years later assuming there had been no prior experience with a lower-cost device?

- What is the nature of the international collaboration program in the stellarator area with Japan and West Germany? What is involved, who is doing what R&D, what devices or other facilities are included and how successful has the program been do date?

- On an international program such as this, how do we assure that industry benefits are derived by both sides?

- Is the D-T "scientific feasibility" experiment at the Princeton TFTR on the critical path for subsequent ignition experiments? Specifically, does the slippage in this program imply a corresponding delay in the time at which the DOE will have achieved the capability to execute an ignition experiment?

- How successful has the international collaboration program been with West Germany, in the area of ash and impurity removal, and how does that work relate to R&D at Princeton?

- What has been the Japanese reaction to the termination of FMIT activity in the U.S.? Aren't the diplomatic impacts of SRC-II and FMIT terminations much more serious for our partners than they would have been if the U.S. were the affected party?

- What impact will the $600,000 reduction in the Radiation Facilities program have on the Japanese government in FY 1986 and beyond? Will it erode our credibility in future agreements further if we reduce our effort in this one?

- Recognizing that there are certain incentives to perform various tasks in the fusion program in cooperation with other nations, why was it not possible to obtain any agreements for further international cost-sharing on the Fusion Materials Irradiation Test Facility?

- What would the design parameters of the Ohmic Heating Toroidal Experiment (OHTE) have to be to sustain ignition?

- Is this device based on the principle of replacing the vacuum chamber after operation, and if so, what is the predicted useful life of a vacuum chamber?

- How would you describe the differences between OHTE and the Advanced Toroidal Facility?

- While the existing Advanced Toroidal Facility (ATF) at Oak Ridge is not intended for and cannot be used to achieve ignition, what is your guess as to the future possibilities of using a second-generation ATF to achieve low-cost ignition in the early 1990's?

- Has there been any computer modeling carried out at the computer center at

Livermore on the Advanced Toroidal Facility, and if so, what was the nature of the modeling, what were the results and what conclusions can be drawn from this data?

- What fundamental "scientific" milestones, whose achievements are essential to the development of fusion energy, were accomplished in 1984? That is quantitatively, what milestones have been achieved, what remains to be accomplished and what are the specific priorities for FY 1986?

- Would you comment on the potential for such toroidal devices as the reversed field pinch, stellarator, and the compact toroid for overtaking the tokamak and becoming attractive as possible fusion reactors?

- What advancements were made in RF heating in 1984?

- Please give us a comparison between the ZT-H experiment and the Ohmic Heating Toroidal Experiment, identifying the similarities and differences between the two.

- The budget refers to new pellet fuel injectors. What is the current state-of-the-art of these injectors and is there any significant research and development work that needs to be done in this area? If so, who is doing it?

- It is conceivable that there will always be new and exciting ideas to harness fusion energy, yet at some point, it will be necessary to select a configuration for a test reactor. Please describe the criteria for making such a selection and how such a program transition can be accomplished in an orderly manner.

- What is your evaluation of the Fusion Research Center concept offered by Mr. Mike McCormack during his testimony on March 8, 1985?

- Please identify the minimum value of each of the following parameters, or combination of parameters, which is necessary to achieve ignition.

  Particle Energy Density
  Temperature
  Stability Factor
  Current Density
  Confinement Time

- Please identify the values of each of the above already achieved on the following devices:

  T-3
  D-III
  PLT
  Alcator C
  JET

listed in the previous question, including all associated operating, PACE, and capital equipment costs.

- How successful has the work carried out at the University of Wisconsin been and how does it relate to other fusion research?
- What will be the leasing costs associated with the new Cray 2 computer to be installed this month? Was the Cray 1-S also leased? If not, where is it being moved and what compensation, if any, will be paid to the government? What is the installation cost of the Cray 2, or is that included in the lease agreement?
- What specific contributions were made by the Magnetic Fusion Computer Center in 1984?
- What progress has been made on the TMXU during the past year?
- How does the TMXU work tie into the the mirror work being done at MIT? How does TARA relate to endplug improvement?
- What TMXU work is planned for the remainder of FY 1985 and how is this work being coordinated with MIT and other fusion subprograms?
- What TMXU experiments are planned for FY 1986, what results are anticipated and how will these contribute to the overall Magnetic Fusion Energy program?
- The budget refers to preparations for limited tritium operations at the TFTR. Will you describe these operations? The budget also refers to attaining energy breakeven conditions in a deuterium plasma at TFTR. What are these conditions?
- The budget book seems to indicate that DT plasmas will not be used at TFTR until some time beyond FY 1988. If that is correct, why is it necessary to do tritium preparation work two years ahead of time?
- What hydrogen plasma experiments are planned for TFTR beyond FY 1986?
- Does DOE have any specific plasma criteria against which newly proposed experiments would be rated before initiating new device fabrication? If so, what is it?
- The FY 1986 budget states that nuclear technology is a "key element in determining the safety, economics and impact of fusion." Would you comment on the suitability of the MFTF-B as a facility for nuclear technology development? Specifically, would it be significantly easier, and therefore less costly, to design, build, and test a small lithium blanket in the solenoid section of the MFTF-B than it would be to undertake a similar task in a tokamak? Would the design and fabrication of remote handling equipment also be simplified and less costly? How would such device planning relate to the overall program schedule?
- The budget states, "the scheduled acquisition and installation of auxiliary components (vessel, magnet, cryogenic, vacuum, computer and power supply systems), which are required to conduct an experimental program, will be postponed to reduce the overall fusion program budget."

GA Technologies

GA Technologies Inc.
P.O. BOX 81608
SAN DIEGO, CALIFORNIA 92138
(619) 455-3000

May 3, 1985

Ms. Debbie Johnson
Subcommittee on Energy Research and Development
B-374 Rayburn House Office Building
Washington, D. C. 20515

Dear Ms. Johnson:

In correspondence dated April 19, 1985, Chairman Lloyd requested information to supplement the record of the Subcommittee hearings on the FY 1986 DOE Authorization Request for the Magnetic Fusion Energy Program. This request took the form of a number of questions. I am pleased to have the opportunity to state my views on these important issues. Both the questions and my responses are attached.

Please feel free to contact me if I can be of any further service.

Sincerely,

Tihiro Ohkawa

May 3, 1985

RESPONSE TO ADDITIONAL QUESTIONS
FOR DR. TIHIRO OHKAWA

Question 1

In the area of plasma confinement, is it correct to state that the objectives of this part of the fusion program are to continue improving the understanding of the following parameters, and if so, what would be the minimum value of each, or in combination, in order to reach ignition?

(a) The particle energy density compared to the magnetic field energy density.
(b) Plasma heating.
(c) Plasma stability.
(d) The plasma current density.
(e) Confinement time.

Response to Question 1

The proposition as stated is largely correct. The plasma current density, however, is more properly deleted from the list; although an important parameter, it is thoroughly understood. It should be replaced by a measure of plasma purity, i.e. the effectiveness of the impurity control approach in eliminating contaminants to the reacting fuel.

When expressed in terms of these parameters, it is possible to quantify the requirements for ignition only in the context of a particular device design approach. The numerical values are strongly dependent on the magnetic confinement scheme employed and also vary to some extent with design details. In the following, it is assumed that a high density, high field tokamak is the reference design.

(a) Beta: 5% (somewhat lower at very high magnetic fields).

(b) Plasma heating: 25 MW of central heating (0 if the higher risk ohmic heating to ignition approach is successful).

(c) Plasma stability: in addition to the beta requirement, the need is for at most a moderate level of fluctuation in the fusion power level for a period of ten seconds.

(d) Plasma purity: contamination levels not to exceed 1% for gases and/or carbon, 0.01% for metals.

(e) Confinement time, more precisely the energy confinement time: 0.5 second at high plasma densities.

Question 2

Is the Ohmic Heating Toroidal Experiment (OHTE) device based on the principle of replacing the vacuum chamber after operation, and if so, what is the predicted useful life of a vacuum chamber?

Response to Question 2

Yes. If a very compact device such as OHTE proves capable of ignited operation, the capital investment in the torus would be a tiny fraction of the capital tied up in the power equipment and the balance-of-plant. This permits a throwaway mode of operation in which the torus replacement costs are in the same category as fuel costs. The useful life is highly design-dependent but is typically on the order of a year.

Question 3

Assuming that a successful lower-cost ignition device is built and operated, then, what would be the next major step on the path towards a peaceful application of fusion energy? How might the next major step differ if a higher cost ($1.5 billion) ignition device were built five years later assuming there had been no prior experience with a lower-cost device?

Response to Question 3

The next major step would incorporate the major fusion technologies into a burning plasma device and would adopt the configuration judged most suitable for power generation. Undertaking such a step now would entail excessive cost for two reasons:

(1) To make allowance for uncertainties concerning the operating point of the plasma.

(2) To incorporate some conservatism in technology owing to a lack of prior integrated test experience.

There would be a further element of technical risk associated with prejudging the outcome of present investigations to determine the optimal configuration.

A successful, lower-cost ignition device would alleviate the concerns associated with point (1) but the program would remain dependent on technology test beds with regard to point (2). A higher cost ignition device would be of more benefit with regard to point (2) and would also reduce the technical risk inherent in the choice of configuration.

Question 4

Can anyone tell us how other on-going materials research that is being performed under other government programs is being incorporated in the fusion program?

Response to Question 4

The fusion materials progrm has benefitted in significant ways from materials research conducted in other government programs. This will be illustrated by citing a few prominent examples; no attempt is made to be comprehensive.

Fission Programs - Fission test reactors have been the primary source of data on radiation damage in structural materials, an issue of major import for long term fusion research. Indeed, a prime candidate class of structural materials, the martensitic/ferritic steels, was selected largely on the basis of data generated in the fission program.

DOE Defense Programs - Most of our detailed knowledge of the properties of tritium, a principal fusion fuel, has come from production reactors. DOE defense programs have also provided information regarding candidate liquid and solid breeder materials for fusion.

DOD Programs - DOD research is largely responsible for characterizing the properties of graphite in various forms. Such materials are widely employed for high heat flux components in fusion experimental device.

Space Programs - Because of a common interest in coatings, a useful interplay has developed between the space program and the fusion program.

FUSION POWER ASSOCIATES
2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879
(301) 258-0545

STEPHEN O. DEAN
PRESIDENT

May 16, 1985

Honorable Marilyn Lloyd
Subcommittee on Energy
  Research and Production
Committee on Science and Technology
B-374 Rayburn House Office Bldg.
Washington, D. C. 20515

ATTN: Ms. Debbie Johnson

Dear Mrs. Lloyd:

Enclosed are the answers to the additional questions you sent to me on April 19. I am sorry for the delay but I have been out of town extensively.

I very much appreciated the opportunity to testify to your subcommittee and thank you for your continued support for the development of our country's fusion energy option.

Sincerely,

Stephen O. Dean

Enclosure

"TO TAKE FROM THE SEA AND TO IMITATE THE SUN"

Answers to Additional Questions
by Dr. Stephen O. Dean

1. Has the pace for investigation of generic plasma phenomena associated with a burning plasma slipped relative to the pace at which the other areas of the Magnetic Fusion program are being pursued? If yes, is this a result of a re-evaluation of the appropriate timing and/or potential benefits to the program, or of unforeseen technical difficulties?

   The pace of the U.S. fusion program and the timescale for producing and studying a burning plasma has for the past five years been limited by the absence of growth in the funds required and not by technical difficulties.

2. Would you comment on the potential for such toroidal devices as the reversed field pinch, stellarator, and the compact toroid for overtaking the tokamak and becoming attractive as possible fusion reactors?

   Non-tokamak toroidal devices may eventually prove to be acceptable or even superior as commercial fusion reactors. The timescale for commercial fusion power is such that it would be premature at this stage to try to pre-judge which concepts (toroidal, linear or even inertial) will eventually prove to be best.

3. Assuming that a successful lower-cost ignition device is built and tested, what would be the next major step on the path towards the peaceful application of fusion energy? How would the next major step differ if the more ambitious, higher cost ignition device were built instead but at least five years later than a lower-cost device?

   There is no unique develoment path to fusion power but several possible ones. Lower cost, earlier steps will result in more steps being required later, leading to longer timescales and higher overall cost of fusion development. Higher cost steps in the beginning would lead to fewer steps, shorter timescale and lower overall cost. It appears that government policies are such that low initial step costs are required even if this leads to higher overall development cost. There is no assurance that a more ambitious step than now envisioned would have an acceptable cost in five years, or even in ten.

4. It is conceivable that there will always be new and exciting ideas to harness fusion energy, yet at some point, it will be necessary to select a configuration for a test reactor. Please describe how such a program transition can be accomplished in an orderly manner.

   Commitments to a new generation of more ambitious facilities must be made every 5-7 years if orderly progress is to continue; otherwise the capability to advance deteriorates as facilities become obsolete. This forces the selection of the "best available concept" at any particular time. At this time, the tokamak is clearly the leading candidate for pressing ahead to produce fusion conditions. This does not mean that there are not many important, less expensive facilities that should be funded for other concepts also.

5. Changing public perceptions for the need to develop long-term abundant energy sources, such as fusion energy, appear to have eroded support for this program. Will you comment briefly on the forecasts for world energy supply into the next century?

   It takes a long time to develop a new energy technology to the point of commercial application and even longer for it to penetrate the market enough to make a difference. Power plant construction lead times are also long compared with characteristic periods of demand fluctuation. In 1980 the total installed electric capacity of the U.S. was 570 GWe. This will have to be replaced during the first half of the next century, in addition to building new plants to accommodate to population growth. One can argue about growth rates but even a conservative estimate like 1-2% would lead one to conclude that something like 20 to 50 new gigawatt level power plants will have to be initiated every year during the first 50 years of the next century. This is a big market, since each plant will cost about $2 billion dollars regardless of technology. Since there are many unresolved social issues associated with the use of fossil and nuclear fuels, the public has a right to expect that new technologies, like fusion, will be developed to the point where a fusion option exists for the above market. Even with an accelerated effort on fusion, it will take 15-20 years to demonstrate the commercial viability of fusion. Thus, in my opinion, it is cost-effective from a public funding point of view to develop fusion expeditiously now.

EBASCO SERVICES INCORPORATED EBASCO
Two World Trade Center, New York, N.Y. 10048

Leonard F. C. Reichle
Group Vice President
Advanced Technology
and Special Projects

May 14, 1985

Honorable Marilyn Lloyd, Chairman
Subcommittee on Energy & Production
Committee on Science & Technology
U S House of Representatives
Suite 2321, Rayburn House Office Bldg.
Washington, D. C. 20515

Dear Congresswoman Lloyd:

This is in response to the two additional questions raised in your letter of April 19, 1985.

Question 1 - "Changing public perceptions for the need to develop long-term abundant energy sources, such as fusion energy, appear to have eroded support for this program (the Fusion Energy Program). Will you comment briefly on the forecasts for world energy supply into the next century?"

Public perceptions regarding the need to develop long-term abundant energy sources, although well intended, are misleading, unfounded on fact, and lulling into complacency fellow citizens who should be urging the support of programs to protect our energy future.

Electricity will supply an increasingly large percentage of total energy supply. In the United States alone, electric energy demand is forecast to increase from 2.5 trillion kwh in 1985 to 5.8 trillion kwh in Year 2030, according to the Energy Information Administration of the U. S. Department of Energy. This demand will require the generation of electricity in an amount equivalent to the output of 1,060 power generating units of 1,000 MWe each. Already, in the Southwest of the United States, some electric utilities are finding reserve electric generating capacities dangerously low.

Currently available sources of long-term energy supply have serious disadvantages. Petroleum and natural gas reserves are limited and can be expected to become more expensive as readily available reserves are exhausted. Moreover, it is highly desirable to conserve resources of these fluid fuels for applications for which they have unique advantages, i. e. for transportation, space heating, and chemical feed stocks. In time, even though shale-oil and oil from tar sands can augment our supplies of natural fluid fuels, their costs at best will be comparable to the costs of oil and gas.

Coal and nuclear fission are abundant sources of electric energy, but safety and environmental problems, although amenable to solution, have produced uncertainty, delay and greatly added cost. This is apparent from the present de facto moratorium on nuclear plants and concerns about "acid rain" and the "greenhouse effect" from coal-fired plants. If further study shows that "acid-rain" and the "greenhouse effect" result in curtailing the burning of fossil fuels (and also biomass), it would leave nuclear fission, hyro, solar and fusion as our only alternate energy options.

Nuclear fission has the limitations of safety and environmental problems discussed above. Hydro power (and wind, tides and thermal gradients) is limited in availability. Solar energy is commercially attractive for certain space-heating and process-heat applications, but solar electricity will be relatively expensive.

This leaves fusion energy as our most attractive alternative energy option, the development of which we should proceed with at an accelerated pace. Fusion is attractive because it:

- Is intrinsically safe
- Has minimum environmental problems
- Uses an inexhaustible source of fuel
- Provides freedom from fuel embargo
- Is potentially a low-cost producer of electric energy

It would be in the national interest for Congress to provide the leadership to develop a public perception that: (1) the demand for electric energy is increasing far more rapidly than for total energy, (2) principal sources of commercial electric energy such as oil and gas, coal and nuclear fission have severe safety, environmental and cost problems, and (3) fusion energy among other energy options is potentially the most attractive and should be developed at an accelerated pace.

Question 2 - "Assuming that a successful lower-cost ignition device is built and operated, then, what would be the next major step on the path towards a peaceful application of fusion energy? How might the next major step differ if a higher cost ($1.5 billion) ignition device were built five years later assuming there had been no prior experience with a lower-cost device?"

The next major step after a lower-cost ignition device is built and operated is to design, construct and operate a Demonstration Fusion Power Plant. TFTR will establish "break-even" and the "lower-cost ignition device" will demonstrate ignition and long-pulse equilibrium burn. The next major step should be a Fusion Power Demonstration Plant, funded by the DOE and managed and technically directed by an industrial company or industrial team (not a national laboratory or university). It should not be a so-called Engineering Test Reactor dedicated to the development of generic engineering systems, materials and components. Instead, it should demonstrate the feasibility of producing fusion-electric power. It should be authorized for engineering to start prior to and construction immediately upon successful operation of the lower-cost ignition device.

If a higher-cost ($1.5 billion) ignition device were built five years later, it would still be necessary to proceed next to a Fusion Power Demonstration Plant. We would be losing 5 years in taking the next step toward demonstration of the feasibility of producing fusion-electric power. So, we should be moving promptly to the authorization of a lower-cost ignition device. The additional knowledge that would be gained from a higher-cost ignition device can be obtained in the more practical environment of designing and building a Fusion Power Demonstration Plant.

Sincerely yours,

Len

LFCR:ia

Grumman Corporation
Bethpage New York 11714

JOSEPH G. GAVIN, JR.
chairman of the executive committee

May 14, 1985

Congresswoman Marilyn Lloyd
Chairman - Science and Technology Committee
Subcommittee on Energy Research and Production
2334 Rayburn House Office Building
Washington, DC 20515

Dear Marilyn:

In response to your letter of April 19, 1985, I am enclosing a series of brief answers to the questions posed. The answers are numbered to correspond with the question. I hope that these personal comments will be of assistance in your consideration of the Magnetic Fusion Energy Program.

Very truly yours,

Joe

JGG:jo

Enclosure

1. Without commenting on the specific program in question, I think the issues of agreements, trust and stability of programs can be addressed. The U.S. is perceived by some as an "unreliable partner" because of experiences in other government-to-government programs such as space science and synthetic fuels. The annual funding appropriation process makes it difficult for the U.S. to have "stable" program commitments when the commitment involves a multiyear program. However, requesting explicit budget items for international projects may ease the problem. Allowances can be made for alterations in agreements and/or commitments by structuring them with possible changes in mind. However, it is essential to avoid unilateral decisions. Consultation with partners must be timely and real.

2. The basic idea behind the lower-cost ignition device is to demonstrate ignition as soon and as inexpensively as possible. If this demonstration is successful in the late 80's, there would be a far greater confidence in proceeding with a reactor class device based on the experience gained in the 85-90 period with TFTR, JET, JT-60 and other contemporary facilities.

   If the lower-cost ignition device is bypassed in favor of a more elaborate and expensive ignition demonstration, the result would be in hand later. This implies that many near term engineering commitments must be made without waiting for final TFTR, JET, JT-60, etc. results. This approach appears to offer higher near term cost, higher risk, and a potential delay to the overall goal of practical fusion energy.

3. (A) Seldom do international issues have a finite starting point. As in the case of MFE, issues relating to large devices have already been discussed through scientific channels as well as political channels. For example, the summit working group in Controlled Thermonuclear Fusion/ Versailles Economic Summit, and the International Fusion Power Coordinating Committee. This activity should be endorsed at political levels and steered by the fusion program leaders in the respective countries, who should meet periodically to reconcile their programs. Subsidiary planning groups,

3 (A) continued

involving technical leaders, should meet periodically to plan cooperative activities. This activity must be a continuing one. The involvement at the technical level is important to both the planning of sound objectives for the project and to the development of a cooperative spirit for its pursuit.

(B) The agreement should be negotiated by a joint State Dept.-Dept. of Energy team after a series of preliminary meetings at several technical and political levels have explored the possibilities and narrowed the question to within reasonable negotiating range.

(C) There are two parts to this problem. The first is getting U.S. industry involved early and vigorously. No agreement can make up for a head start such as Japanese industry seems to be getting. The second part concerns ownership of intellectual property. Any international agreement should provide for patent protection and cross-licensing.

(D) European consortium would not necessarily need U.S. participation. Technically and politically they are capable as exemplified by JET.

(E) The ideal arrangement should be a multilateral program, so as to involve all interested parties. This would maximize the spread of science, technology and cost. However, ideal arrangement are not always the most desired arrangement by all interested parties. For example, the Japanese seem to prefer bilateral agreements. When given the choice, they seem to prefer the U.S. to the Europeans. Geography and economic conditions may be reasons for this preference.

(F) Involving other countries in the program is very likely to increase the cost of the device and the time required to build the device. However, one should consider the alternative: Can one country afford to build the next device(s) required to reach commercialization? Shared cost would be less to any individual country in the given scenario.

(G) Being the host for the International Device has some advantages and disadvantages associated with it. A significant advantage is the avoidance

3. (G) continued

of sending scientists, engineers (and families) overseas. Among the disadvantages are the cost of being host and providing for relatively long term visitors. A review of the JET experience suggests that the host country's advantages are mixed.

(H) I am convinced that trying to separate the scientific and engineering aspects of fusion research is counterproductive. I recommend a major role in both areas--perhaps as a leader among equals.

MCDONNELL DOUGLAS ASTRONAUTICS COMPANY
ST. LOUIS DIVISION
Box 516, Saint Louis, Missouri 63166

16 May 1985

Honorable Marilyn Lloyd
Chairman, Subcommittee on Energy Research and Production
House Science and Technology Committee
B-374 Rayburn House Office Building
Washington, D.C. 20515

Dear Madam Chairman:

Enclosed are the answers to the supplementary questions you had requested the IEEE to answer in regard to Fusion Energy R&D. Please let me know if I or the Institute can be of further help to you.

It was a pleasure to testify before you again this year.

Sincerely,

Allan T. Mense

MCDONNELL DOUGLAS
CORPORATION

Supplemental Questions for Dr. Allan T. Mense

Question #1

- Do you know if the Rotating Neutron Source (RTNS) work at the Lawrence Livermore National Laboratory is sufficient in scope and facilities to provide the experimental data essential to meeting program objectives? Is there any device beyond RTNS, but one that would be less costly than FMIT, which could more closely simulate the fusion environment?

I have chosen to break the answer to this question up into two parts.

- Is the Rotating Target Neutron Source (RTNS) work at the Lawrence Livermore National Laboratory sufficient in scope and facilities to provide the experimental data essential to meeting program objectives?

  The answer depends upon the definition as to the type of experimental data that is needed to meet the program objectives. Currently the RTNS facility is the only pure 14 MeV neutron source available in the US and as such is capable of producing the type of damage to materials that is anticipated in a fusion reactor (dpa + transmutation products). Unfortunately the flux which is approximately 10^{13} n/cm^{2}s will only produce roughly 1 dpa/year, assuming continuous operation for a full year. For stainless steel a 1 MW/m^2 wall loading will result in approximately 11 dpa/yr. Therefore unless this machine can be upgraded to produce a FMIT type of flux it is not useful for generating materials property type of data and it's because of this that the national and international materials programs still rely on currently available fission sources. However, since the RTNS is a pure 14 MeV neutron source, it is useful in measuring damage cross sections of various materials which increases our predictive capability as well as helps our understanding of the fundamental aspects of radiation damage to materials. The real question that should be addresses is if the knowledge gained is worth the cost of operation. The answer is probably not.

- Is there a device beyond RTNS, but less costly than FMIT, which could more closely simulate the fusion environment?

  At the present time, no. There are a number of scientists who have suggested alternatives to FMIT but none of the concepts or modifications appears to be able to completely duplicate the proposed capabilities of FMIT. For example, LAMPF located at LANL has been suggested, with modification, as a possible test facility for fusion. The projected damage rate would be on the order of 14 dpa/yr using a uranium target and operating the facility continuously. The test volume would be several liters and may approach 59L. The high flux region of FMIT would produce roughly 50 dpa/yr under continuous operation. Therefore, the trade is do you want test volume or end of life testing? Since many property changes do not occur until the material dose reaches roughly 15-20 dpa, it would take two years of operation of LAMPF. For swelling, many of the candidate materials do not reach incubation until >100 dpa and as a result only a fission reactor or a FMIT facility has this type of capability. Therefore, the LAMPF facility does not appear to be useful as a materials test facility.

However, because of its large test volume, it could be useful for testing some of the subscale or component tests suggested by the FINESSE study. It is important to note that before the LAMPF can be considered, the source will have to be spectrally mapped to see if there are regions free of the 100 MeV neutron tail caused as a result of the spallation.

Compact reactors such as the Riggatron have also been suggested but in these cases it is not clear if the costs will really be less than the FMIT and these machines have the additional challenges of physics to overcome before they can be considered as reliable test facilities.

Question #2

o Describe the materials work that should now be in progress to meet the fusion requirements of the future?

- This is not an easy task and presumes that we know the direction that fusion will take in the future and what type of machine are either going to ignite or will be useful commercial concepts. The argument is whether the program should be broad based to cover all options or be aggressive and home in on one favorite. Regardless, with the approach used, no one will be happy with it. Logic favors the former because it provides the reactor designer with as many options as possible to tailor his designs to the customer requirements. Ideally there will not be one unique fusion machine but there will be several possibilities similar to light water reactors that will meet the customer needs whether they be low activation or low cost. At the present time, effort should be directed toward scoping the response of materials to radiation damage. Emphasis should be on mechanical properties to enable calculations on the structural life of components. First wall life is important but work on other components is also important. Material work should not be reactor specific (with the exception of compacts) but should address generic issues such as high flux components, resistive magnets, radio frequency heating systems, and electrical insulators which do not degrade in intense radiation environments.

Question #3

o What work is planned for the TFTR in FY 1986 that will lead to plasmas which more nearly approximate reactor-relevant conditions?

The TFTR has already achieved operation at full design values of plasma current (2.5 MA) and magnetic field (52,000 gauss). With neutral beam heating power of 6 MW an unprecedented volume of near reactor-grade plasma has been produced. The machine is now being readied for the installation of additional neutral beam heating apparatus to more than quadruple the heating power. This is expected to produce reactor-grade plasma in FY 86 and allow demonstration of plasma conditions corresponding to energy breakeven (in DD plasma) in the following year.

Question #4

o Is the D-T "scientific feasibility" experiment at the Princeton TFTR on the critical path for subsequent ignition experiments? Specifically, does the slippage in this program imply a corresponding delay in the time at which the DOE will have achieved the capability to execute an ignition experiment?

Yes, but the undertaking of design need not await these experimental results. It would be extremely useful to begin the Title I design and R&D for long lead time items as soon as possible. This would allow for a title II go-ahead in 1988 to 1990 time frame. The administration, for reasons which are not understandable to the technical community, refuses to even consider the idea of constructing an ignition experiment as a follow-on to TFTR. If the administration's position is followed, then one is never going to have an ignition experiment and therefore the question of "slippage" in the program is irrelevant.

Question #5

o Assuming that a successful lower-cost ignition device is built and operated, then, what would be the next major step on the path towards a peaceful application of fusion energy? How might the next major step differ if a higher cost ($1.5 billion) ignition device were built five years later assuming there had been no prior experience with a lower-cost device?

There is no unique manner of characterizing the steps necessary to prove fusion practical, but one scheme is the sequence: Scientific feasibility, engineering feasibility, economic feasibility. The TFTR will probably prove scientific feasibility, i.e., a controlled fusion reaction is physically possible. An ignition experiment will probably demonstrate engineering feasibility, i.e., a machine can be built that will provide a net useful energy output for a significant period of time. The final step, economic feasibility, must address operational reliability and require demonstration of sustained, long-term operation - a so-called "demo" reactor. The difference between stages two and three is the difference in operating time of several seconds to several months (years?). This is a big spread, and experience with a "low-cost" ignition device would probably require a high cost version before going to the "demo." Why not go directly to an adequate ignition device?

It is important that the country undertake an ignition experiment. The major problem seems to be whether one can do this within a fixed or diminshed annual budget. The answer is yes if it can be done with low budgets but it would draw the project out in time. It makes little sense to commit to an ignition machine that is a small step beyond TFTR, JET, or JT-60. Also there would be little incentive to have international cooperation on such a reduced mission device. If international cooperation is seen as a must in order to share funding, then only an extremely aggressive physics and engineering machine (such as an ETF) makes technical and political sense.

PROMETHEUS II Ltd.
Bx 222, College Park
MD 20740
(301) 445-1075
May 9, 1985

The Honorable Marilyn Lloyd, Chairman
Subcommittee on Energy Research and Production
Committee on Science and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Representative Lloyd:

Thank you for your interest in my comments expressed in your letter of April 19, 1985. I am pleased to respond to your additional questions. If you need additional clarification or backup materials, or if I can be of service in any other way, please feel free to contact me.

Cordially,

Paul M. Koloc

Paul M. Koloc
President

PKoloc:dc

Additional Questions for Mr. Paul Koloc
QUESTION 1

What is the current state of development of your PLASMAK(tm) fusion concept?

The PLASMAK(tm) reactor concept is a natural extension of the RFP and especially the Spheromak. In fact, development of PLASMAK(tm) technology started with the Spheromak, since the natural first step in implementing a careful approach to PLASMAK(tm) technology development from the level of tokamak technology was to form the internal plasma ring of a PLASMAK(tm) plasmoid or PMK, which is exactly the Spheromak plasma.

The cost in time and money of fusion development is in proportion to sheer physical size and complexity. The Spheromak, and to a much greater extent PLASMAK(tm) technology, reduce the cost and development time of fusion to a very small fraction of the expected and realized cost of tokamak and mirror devices.

An example of a simplification and an engineering benefit not shared by other fusion approaches is as follows. The PLASMAK(tm) configuration incorporates the concept of a liquid density gas blanket that surrounds and supports the plasma mantle (cover or "first wall"). Both the plasma wall or mantle and the blanket gas are impervious to the fusion heat released by the reacting core.

The time for PLASMAK(tm) research and development has come. It now has a much broader base of support due to the preliminary research and development program of the Spheromak. The basic science learned from these Spheromak experiments is that:

a) MHD stability requires a tightly fitting conducting shell;

b) Magnetic coil confinement is destablizing;

c) Fast startup leads to a cleaner, hotter plasma;

d) Fast ohmic heating is destabilizing.

Fortunately, a PMK has high conductivity and a tightly fitting conducting shell, and so avoids excessive ohmic heating and does not require external magnets. In addition, the results of the super H mode operation on ASDEX, a German tokamak as reported by Wagner of Max Planck Institute for Plasma Physics, and also studies on energetic electron plasmas by T. K. Chu of Princeton's Plasma Physics Laboratory, seem to confirm the viable existence of a non-diffusing plasma with relativistic currents flowing at the plasma surface and even within the plasma body.

Present efforts include a jointly cooperative effort between Prometheus II, Ltd. and the Laboratory for Plasma and Fusion Energy Studies at the University of Maryland, to numerically compare the stability, energy confinement, and range of compressibility of both a PLASMAK(tm) plasmoid or PMK, and the Spheromak. Analysis already indicates that the PLASMAK(tm) will remain stable and suffer no loss of energy confinement time throughout the compression phase. Furthermore, it has a much larger compression range so that it is a candidate for operating as a hydrogen boron fueled reactor, thus avoiding the havoc caused by neutron bombardment from less exotic fuels.

The state of progress can be measured in terms of time from a commercial ignition experiment. A formation "proof of principle" would only require one or two years' work. An ignition and commercial burn would only require an additional two to three years. Unlike the tokamak, which only allows one thousandth of the applicable pressure to reach the plasma, the PLASMAK(tm) reactor actually multiplies it by 10 or fifteen times. Consequently, there is no need to do a preliminary "scientific breakeven" experiment. Cost of the ignition work would not exceed five million per year; it would not go on without a commercial burn for more than three to five years.

The following has been accomplished:

a. The prerequisite for strong industrial backing of a new venture is the protection of the technology by patents. PLASMAK(tm) technology is protected by a very strong patent package both here in the United States and in most of the influential countries of the world.

b. The concept squarely satisfies the following criteria:

for stability (elucidated by M. N. Rosenbluth)

for engineering correction of the wall problem (elucidated
by L. R. Lidsky)

for simplicity (elucidated by R. L. Hirsch)

c. Support in terms of experimental equipment has been given, and further facilities and experimental costs are expected in fiscal 1986. Prometheus II Ltd. has already received the expressed interest and willingness to participate in PLASMAK(tm) technology development from professionals representing a deep cross section of disciplines from nearly all of the national laboratories and universities presently participating in fusion research. The scientific group includes experimentalists, theorists, and engineers. Similarly, a corporate group will provide a range of expertise in management, finances, engineering, and manufacturing or fabrication of specially needed hardware.

d. Preliminary mathematical analysis has already yielded very exciting results. For example, mechanically driven fluid compression will not adversely affect the critical energy confinement time, as does auxiliary heating of tokamak plasmas. There appears to be at least a temporary bureaucratic roadblock to utilizing funds set aside by Congress for small business use of the Magnetic Fusion Energy CRAY super computer at LLNL. Efforts to obtain its use are in progress.

e. A proto experiment has been fabricated. The first attempts at PMK formation are planned for this summer.

At this point, I would like to make a few additional comments which have a direct bearing upon the present and future of the plasma and fusion science program.

Interactions with the DOE and national laboratories, while stimulating interest and work on the Spheromak and recently the PLASMAK(tm) concept, has given insight into the strengths and weaknesses of the policies followed by the DOE Office of Fusion Energy in operating its fusion research and development program. There seems to be a serious flaw in the review process used by OFE. It doesn't seem capable of starting new concepts from scratch, and it doesn't seem capable of stopping costly, unattractive or obsolete concepts with a very low probability of commercial success.

By analogy, the development of fusion and plasma science can be thought of as being the extension of a surface such as the top of a conference table. What science is known is represented by the area of the table. A brief case might represent a fusion concept, and when tossed upon the table, where it ends up resting would determine how much science there is to support it. Now funding a concept that is "in the middle of the table" will not be very fruitful since there is not much to learn, although probability of experimental success is extremely high. But "the taxpayers money wouldn't be wasted" on an unsuccessful or risky experiment(??). If the brief case lands near the edge but doesn't "hang over", then it too can be funded, and there is a likelihood that at least a few new and interesting things can be learned which will extend the width of the table.

Ideas that slide across the table and "fall off" the edge are "crackpot" and totally unrelated to what has already been accomplished; they would be outside proper consideration for an orderly, well managed government research program. Such work is for the speculative investor, or perhaps the government in a dire circumstance such as the difficulties faced during WW II. Tossing out the ideas that "fall", in a situation where there are very few "really new but related" concepts, it seems quite proper to consider doing exploratory research on the ideas. The greatest advances can be made if they clearly "stay on the table " but "hang substantially over the edge". Even if they "don't work", all is not lost, because it tells you the answer is in another direction. If they do work, a rapid and great advance in plasma and fusion science has been made, and fusion will be a fact or at least at near hand. Such ideas must have a "promise" of being a simpler and generally more attractive reactor potential.

The PLASMAK(tm) concept is a perfect example of this. For example, this configuration is a full two generations beyond the tokamak, and one generation beyond Spheromak (Hirsch's present favorite). A fully pressurized PLASMAK(tm) or PMK plasmoid can generate a million times the power density of a "Starfire" class tokamak. Furthermore, it doesn't have the first wall problems** of other approaches or even the MHD instabilities found in some Spheromak devices. Finding this innovation with its spectacular engineering advantages is very exciting. But unfortunately, it hangs over the edge, and although its center of gravity is over the table, nevertheless OFE will not fund such a "risky" concept.

The potential benefit of the technology far exceeds the promise of other approaches. This technology sits squarely at the apex of the evolutionary developments of other fusion devices. It is difficult to justify the contemplation of a 1.5 billion dollar ignition experiment with a device which has no hope of commercial use, when much more advanced and commercially attractive features could accomplish a more spectacular commercial burn well within five years time, and for only a few millions of dollars.

Once commercial breakeven is demonstrated, a multicorporation effort would be needed for early development. Companies such as McDonnell Douglas, Westinghouse, Litton Industries ,Titan Sytems, The BDM Corporation and Combustion Engineering would benefit greatly by assuming the various roles necessary to bring this technology to full use in its various applications. Restricted patent licensing would help in distributing and managing the vast economic growth this new industry would have. The simplicity of this technology and built-in safety factor would allow significant military and commercial use well within ten years of the first breakeven test.

QUESTION 2

Will you describe the materials work that should now be in progress to meet fusion ignition and test reactor requirements?

Although PLASMAK(tm) technology "solves" the wall problem by using a liquid density gas blanket, and avoids the burning of neutron producing fuels for most applications, it will heavily rely on high pressure materials to form the gas compression shell walls. Energy conversion only requires current state-of-the-art super conducting magnets. In this application, the magnets will not suffer the shear stress, and consequently today's technology is already more than adequate to do the job. It is expected that all "high burn density" devices will require dense plasma / gas blankets.

This is not the case with obsolete approaches such as mirrors and tokamaks. But to spend significant amounts of money for materials research to support non-commercially viable concepts doesn't make any sense. Technology supportive of a tokamak or other "solid first wall devices" appears at this time to be wasteful, or at least premature.

It is too early to make a determination of what (if any) new materials technology will be required, since no clear device can yet be considered as consensus candidate for a commercial thermonuclear reactor. Therefore, any work done should be only to take advantage of or "ride piggy back on" the continuing studies under way in the nuclear fission industry.

QUESTION 3A

Assuming that a successful lower-cost ignition device is built and operated, then, what would be the next major step on the path towards a peaceful application of fusion energy?

If the device were a tokamak and the lower-cost in the one third to half a billion dollar range, the next major step would still require an expensive 1.5 billion dollar plus ignition experiment. Such a low cost tokamak ignition experiment would yield little useful engineering data, and it would essentially have to be repeated in a larger scale experiment. It is the engineering data, confirmation of the wall problems predicted by Lidsky and known by the community, that would be the final nail necessary to convince it is time to get on with more appropriate devices. One hopes that the "300 or 400 million dollar" ignition tokamak wasn't just bait for the more costly 1.5 or 2.6 billion dollar Princeton "chance of a lifetime" big physics experiment.

If, on the other hand, the ignition experiment were a ten million ampere RFP, then one hundred twenty to one hundred fifty million dollars H. Driecer says sixty million) is all that is needed and a much larger step forward will be made, just due to the fact that it is much "closer to the edge of the table".

Also under this scenario, much work on Spheromaks and PMK's could be done with half of the saved "low cost tokamak" ignition, so that both of these would be close to ignition about the same time, or sooner in the case of the PMK PLASMAK(tm) technology. Cost for a Spheromak ignition experiment is about fifty or sixty million and for a PMK commercial ignition the cost is anywhere from five to fifteen million dollars.

In the latter scenario commercial development would be the next major development and no fewer than three devices could well be candidates.

QUESTION 3B

How might the next major step differ if a higher cost (1.5 billion) ignition device were built five years later assumering there had been no prior experience with a lower-cost device?

The next major step under these circumstances would be to immediately abandon the ignition concept completely and move into a more advanced, less costly and more commercially attractive device.

To consider a 1.5 billion dollar ignition device is to rule out for contention and further funding the device and its similar physics. Cost is the expression of the engineering judgement of the workability of a fusion devices. That means that the tokamak or mirror or RFP or whatever the ignition device just plain doesn't work, in the engineering and commercial sense.

Toward the end of the first question, the difficulty of the present OFE policy to terminate big experiments was discussed. The solution to this termination problem has very much to do with this question. The present DOE OFE approach to have concepts satisfy peer (physics) review, tends to keep them going forever. Physics reviews do not directly concern themselves with engineering problems. Nor are they capable of starting new and workable concepts from the exploratory level.

This approach can be compared with driving a standard shift car. Driving only in second gear would cause problems in starting the car to get moving from a stop, and once the car was really moving, the engine would be racing and ready to explode. That's pretty much what has happened to the fusion vehicle.

First gear is for a two or three year exploratory research which only requires a more attractive fusion commercial potential and that the physics of the concept is not well understood. (If the physics were well understood then why do it?) The concept should be "positioned" in an area which seems to be along the direction in which the most successful aspects of previous concepts seems to point.

Second gear is already in place and has benefited so far by the virtue of work started or invented elsewhere, i.e. the Russian tokamak and mirror devices.

Third gear comes in after the "physics" criteria have been in place for ten years, or not more than thirteen years from the initial exploratory experiment. In this phase "gear" the concept is tossed out if the engineering problems make it look "uncommercializable" or more simply put "it costs very much too much".

If an ignition experiment would cost over three hundred million dollars, that means that the concept is too costly and by definition fails the principal engineering criterion.

It may well be the task of Congress to reinstate the role of open science in the program and to impose common sense in device selection, by direct law or budget reallocation or modification. For example, a lower budget will force out the obsolete "big" devices and will favor a much larger number of lower capital cost devices. Because of less complexity, a faster experimental turnaround time would insure that more plasma science and fusion progress would be made, and more interesting plasma science and resulting fusion reactors would be forth coming.

Making a joint venturing fund available for private industry and opening national laboratory facilities and making space available for industry's participation on an initiative basis, will dramatically improve the progress for fusion. The motto of Prometheus II, Ltd. is "Fusion This Decade".

This completes my answers to your questions. Thank you again for your interest and the opportunity to respond to your very excellent investigations.

Plasma Fusion Center
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Ronald R. Parker
Associate Director

Telephone 617/253-5553

June 4, 1985

Ms. Marilyn Lloyd
Chairman
Subcommittee on Energy Research
and Production
U.S. House of Representatives
Committee on Science and Technology
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Ms. Lloyd:

Enclosed are my responses to the questions which you addressed to me in your letter of April 19, 1985. I note that my reply is late in coming, and I can only offer the excuse of the crunch imposed by end of term business at my university.

Your questions are very comprehensive and insightful, and I hope that my comments will help you and your committee in your deliberations. Your continued support of the U.S. fusion research effort is greatly appreciated.

Sincerely yours,

Ronald R Parker

Ronald R. Parker
Associate Director
Professor of Electrical Engineering

rrp/lp

Enclosure

Response to Questions for Ronald R. Parker
by the House Subcommittee on Energy Research and Production

1. Reductions in the fusion program budget have a double impact on the training of graduate students. First, fewer students can be supported as research funds decrease; and second, the quality of the students entering the fusion program diminishes, since the students are fully aware that a declining budget signifies a lack of commitment on the part of government to the development of fusion energy.

2. It is indeed correct to state that the objectives of the confinement part of the fusion program are to gain increased understanding of items a), b), c) and e). The plasma current density is not a specific item of importance except as relates to plasma stability (c) and plasma heating (b). To this list, I would add an item called plasma purity, since creating pure (i.e., only hydrogenic) plasmas is very important for fusion applications.

As I stated in my testimony, the most cost effective route to a test of the behavior of an ignited or self-heated plasma is using the high-field, relatively compact tokamak. For this device, the necessary parameters required for ignition are:

a) plasma "Beta" of about 5%;

b) plasma heating power of 10-20 Megawatts;

c) plasma stability permitting only one "disruption" per several hundred ignited discharges;

d) no criterion need be imposed on current density;

e) confinement time in the range 0.5-1.0 seconds;

f) plasma purity as measured by the "effective charge", $Z_{eff} < 1.5$. (Less than 1% light elements such as carbon, 0.1-.01% heavier elements such as iron.)

3. The major advance in RF heating made during the last year was production of temperatures in the range 50,000,00-60,000,000 °K using approximately 5 Megawatts of power at the Ion Cyclotron Frequency. This experiment was carried out on the PLT tokamak at Princeton Plasma Physics Laboratory. These results support the concept of using this method of heating, which is relatively inexpensive, to produce ignition in the tokamak ignition experiment.

4. Pellet fuel injectors have now been developed which can continuously fuel large tokamaks such as TFTR by use of injection of frozen deuterium or tritium pellets at speeds approaching 2 km/sec (slightly more than 1 mile/sec). These injectors need more R&D in order to be capable of optimum fueling for the tokamak ignition device. Development of pellet fueling injectors at the Oak Ridge National Laboratory has been an outstanding technological success of the U.S. fusion program.

5. This is a very broad question and has just been extensively considered by a specially convened MFAC panel. I would make the following brief comments.

First, each of the concepts listed have individual qualities which in some way are more desirable relative to the "standard" tokamak reactor concept. For example, the RFP requires a much more modest toroidal field magnet, the stellarator is a true steady-state device, etc. Nevertheless, we must not lose sight of the fact that reactor size is extremely important from the point of view of reactor economics and plant capacity. The size is mainly determined by confinement properties, and tokamaks have much superior confinement to any other concept. The stellarator is the next best, and may ultimately prove to be the ideal toroidal reactor. Much more work needs to be done on this concept before such a judgment can be made. In my opinion, the confinement properties are the most fundamental attributes of a given concept. We are wasting time and resources on developing those concepts which do not have the potential for adequate plasma confinement.

Second, when comparing concepts, it is important to take into account improvements which can and will be made on tokamaks. For example, there is no theoretical reason why tokamaks cannot operate at plasma beta values of 20-30%. This development would greatly reduce the complexity of the toroidal magnet and offset many of the potential advantages of RFP's. Similarly, RF current drive continues to be developed on tokamaks. An efficient RF current drive would allow the prospect of a steady-state tokamak reactor, and this would offset some advantages of the stellarator.

Finally, as each concept is developed, more of the warts on it begin to show. The tokamak is by far the most evolved concept and so its warts are highly visible. As each of the alternate concepts is developed, we will begin to see more clearly limitations and disadvantages relative to the tokamak. I do not advocate narrowing down to research only on the tokamak. But I do believe that alternative concepts should only develop at a pace which is justified by confinement results.

6. After a successful ignition experiment, the next large step would be a long-pulse, ignited tokamak in which fusion technology and engineering development would be a major objective. In many ways, such a device would be prototypical of a tokamak fusion reactor core.

By taking an inexpensive ignition step, the next step could focus more on technological and less on plasma physics issues. Further, the design could be optimized for performance based on what is learned in the ignition step. If the ignition step were not taken, then the technological attributes of the next large step would have to be compromised owing to the uncertainties associated with the physics of ignition.

7. I am not in a position to comment on how materials research supported by other government programs is being incorporated into the fusion program. Two people who might be able to address this question are:

Dr. Charles Baker
Argonne National Laboratory
Fusion Power Program
9700 S. Cass Avenue (Building 205)
Argonne, Illinois 60439

Dr. Gerald Kulcinski
Sterling Hall, Room 443
1150 University Avenue
University of Wisconsin
Madison, Wisconsin 53706

TRW

Energy Division
TRW Space & Technology
Group

One Space Park
Redondo Beach, CA 90278
213 535 4321

85.T700.PS-078

1 July 1985

Ms. Debbie Johnson
U.S. House of Representatives
Committee on Science and Technology
B-374 Rayburn House Office Building
Washington, D.C. 20515

Dear Ms. Johnson:

Please find enclosed answers to the questions posed by Marilyn Lloyd, Chairman of the Subcommittee on Energy Research and Production.

We at TRW are pleased to have this opportunity to respond to the subcommittee questions and hope that we are contributing to the legislative process.

Yours truly,

Peter Staudhammer

Peter Staudhammer
Manager
Energy Systems Operations
TRW Inc.

/jkk

Enc.

Supplemental Questions

U.S. House of Representatives
Committee on Science and Technology
Hearing on FY1986 Authorization Request for the
Magnetic Fusion Energy Program

March 18, 1985

Submitted by

Peter Staudhammer, TRW, Inc.

Question 1

The FY 1986 budget states that nuclear technology is a "key element in determining the safety, economics and impact of fusion." Would you comment on the suitability of the MFTF-B as a facility for nuclear technology development? Specifically, would it be significantly easier, and therefore less costly, to design, build, and test a small lithium blanket in the solenoid section of the MFTF-B than it would be to undertake a similar task in a tokamak? Would the design and fabrication of remote handling equipment also be simplified and less costly?

Answer

The provision of a facility for nuclear technology development is indeed a key element in determining the safety, economics and impact of fusion. Because of its importance to the fusion program, studies of such a facility based on both toroidal and linear geometries have been made. These studies have indicated that an up-grade of the current MFTF-B facility would meet practically all requirements for nuclear technology at a modest cost.

Tandem mirrors are unique in their projected ability to produce high power density and wall loading at modest fusion power and cost. Their uniqueness is that arbitrary length neutron producing regions can be inserted in the central cell of the machine. Driven to high fusion power density by neutral beams similar to those in MFTF-B, the physics performance of this test section would be assured if the present MFTF-B goals are met. Therefore, MFTF-B would be ready for integrated nuclear technology tests in the 1990's. This would follow a near-term program of separate effects testing using point neutron sources, accelerator-based sources, or fission reactor in-pile testing. Integrated testing of blankets to fluence levels sufficient to resolve many of the critical technology issues would be carried out in up-graded MFTF facility.

A conceptual design of an MFTF-B upgrade shows that 2 MW/m^2 wall loading can be achieved in a test cell in the solenoidal section of the machine, producing 11 MW of fusion power. Comparable wall loadings could only be achieved in tokamaks if accompanied by the production of several hundred megawatts of power.

A recent study at UCLA, with broad national and international participation, found that there is a need for such integrated testing. The conclusion was that an ideal facility would have wall loading, fluence, test area, and fusion power levels close to those that the MFTF-B upgraded facility could provide.

Remote maintenance should be less costly than for a tokamak, and somewhat simplified. This follows from two salient features. First, there are far less total neutrons produced because only a small fraction of the machine operates at high wall loading. (The remainder operates at 10-15% of this peak value.) And, second, linear geometry requires of less sophisticated maintenance equipment. In the test cell, blankets are removed by simple vertical lifts.

Finally, it should be noted that as proposed, the MFTF-B upgrade would have a dual physics/technology role. The total cost is roughly the same as the original MFTF-B facility (less than $500 million), and only a third of that is the add-on for technology purposes. Thus, the cost benefit is high and one is not risking a large investment in a dedicated new facility to accomplish the integrated blanket tests.

Question 2

If you are familiar with the Japanese fusion program, can you tell us if their program is structured in a way which might benefit their industry and how it would? Is this a good model for the U.S.?

Answer

The Japanese fusion program has established a cooperative arrangement with Japanese industry that benefits both. The fusion science program is performed by universities and national institutes. Fusion devices are built and the fusion technology program is performed by Japanese industry. It is our understanding that when construction of the major devices is complete, Japanese industry will continue its role in fusion through operational support. Through this arrangement

Japan is both using its best science talent and building an industrial base. The role of industry is further assured through the budget allocation process. Funding for industry is separate at the national level from funding for the science program.

In contrast the U.S. fusion program has been funded and managed on a project basis, with no a-priori distinction between national institutes and industry. The U.S. approach has the advantage that it allows full project focus, but has the major disadvantage that it does not provide for any continuity in industrial development. This disadvantage is particularly apparent now, in a time of tight fusion budgets. Much of the industrial capability developed over the past decade is being lost due to lack of continuity.

A good model for the U.S. fusion program would be an approach based on elements of both the current U.S. and the Japanese programs. As in the current U.S. program, the focus on projects should be maintained. As long as the projects remain primarily science oriented, management should remain through the national fusion centers, as it is today. However, these fusion centers should seek and rely on support from U.S. industry. The support should be meaningful in contributing to the development of fusion, rather than delivering standard components and providing standard support. Industry should also have the responsibility for technology development, with the national program assuring continuity.

An excellent model for industry involvement already exists in the U.S. in the MFTF-B project, wherein Lawrence Livermore National Laboratory shares with TRW in the development of subsystems and in operation of the facility. The industrial involvement is meaningful in content and establishes a long-range industrial capability which will be useful in the future.

In summary, the current U.S. fusion program does not focus on the development of an industrial capability, whereas the Japanese program does. An ideal model would be an approach incorporating elements of both. Such a model is being implemented today on MFTF-B. Unless the U.S. adopts a policy of industrial capability development, such as on MFTF-B, U.S. industry will be at a definite disadvantage when fusion moves into the engineering and deployment phases.

Question 3

> Would "in-pile" testing, that is, in a fission reactor, make any sense in developing superconducting materials for the magnetic fusion environment?

Answer

Testing of superconducting materials in a fission reactor would be very useful for the development of radiation resistant superconductors. Design studies are already showing the benefit of radiation resistance in lowering the mass of a fusion plant, and hence its projected economic competitiveness. The materials of interest include electrical and thermal insulation and stabilizer as well as the conductor. Moreover, the large volume available in fission reactors could allow tests of a scaled down operating coil, including heat removal and recovery from normal zones.

Although there are significant differences in the neutron energy spectrum between fission and fusion reactors, in-pile fission reactor testing is the only reasonable alternative for the fusion program to get timely data. Comparable fusion facilities will not be available for 5 to 10 years, even under the best of projections for a fusion engineering test facility. Reliable data for fission reactors could simplify the design and reduce the cost of any fusion engineering test device and of subsequent fusion reactor.

Question 4

> What activities (progress) has been made on the TMX-U during the past year?

Answer

The TMX-U was designed to achieve two principal objectives:

(1) Investigate a complete thermal barrier tandem mirror system. This involves demonstration of end plug microstability, barrier and plug electron heating, plug to central cell temperature differences and MHD stability.

(2) Improve TMX performance.

The first objective has been met for densities up to 10^{12} per cubic centimeter. So far the second objective has been met, but so far only in the low density regime.

Specific achievements of TMX-U have been:

- TMX-U operates MHD stable.
- The existence of thermal plugs has been demonstrated.
- Axial confinement times of 100 msec has been achieved.
- Radial transport control has been demonstrated with floating end plates.
- Energy confinement time needs to be further improved, particularly at high density, but the major factors affecting confinement time are understood.
- In the end-plugs, hot electrons and sloshing ions, both necessary for axial confinement, have been demonstrated.
- High efficiency coupling of ECRH and ICRH power to the plasma has been demonstrated.
- Beta values of 12.4% on axis and 5.2% average have been attained with neutral beam heating.
- An extensive physics and technology program is supported as part of TMX-U, which provides much of the underpinnings of not only TMX-U, but of the entire mirror fusion program.

A more detailed summary of the TMX-U results, in more technical terms is enclosed as Appendix A. The Appendix presents the executive summary of the TMX-U Data Review of February 1985, supplemented by a summary of data since that time.

In general, TMX-U has performed very well and continues to look for further improvements in mirror performance. The facility is currently down for a combination of routine maintenance and for modifications. Plasma experiments are expected to resume in mid-August of this year.

Question 5

> How would you describe the present state of engineering technology, that is, are there any engineering constraints which could inhibit the advent of ignition? If so, what are they?

Answer

Confidence in the achievement of ignition with the present state of technology is very high. The achievement of ignition requires only moderate extrapolations in technology. The key areas are the first wall, impurity control system and magnets. The degree of engineering extrapolation is inversely related to the cost of the device. To achieve low cost, engineering designs have been developed that have higher stresses, use less well-characterized materials and incorporate new design concepts that need testing and validation.

Over the past year, we at TRW have studied the design of ignition experiments very extensively. Our conclusion is that an ignition device could be built for less than $200 million. The device concept calls for a compact, high field configuration. Existing scaling laws, validated by all major experimental devices, indicate that ignition will be achieved by ohmic heating alone. To further assure ignition and to provide experimental flexibility, auxiliary heating is provided through ICRH. Because of the high coil stresses required in this concept, a set of validation tests are recommended as the first step in the design and construction of the device. With the verification tests, all known elements of risk will be eliminated.

Question 6

> Is American industry withdrawing from the fusion program, and if so, what suggestions would you have for arresting this withdrawal within the present budget constraints?

Answer

As one examines practically any measure of industry participation, it is apparent that the role of industry in the fusion program is diminishing. The diminishing role of industry is due to two factors, the lack of continuing contractural support and the lack of any national resolve to develop fusion as an energy source. As fusion budgets have decreased in recent years, the fraction of funding available to industry has decreased even more sharply. In some areas, such as superconductive magnets, funding has practically stopped and U.S. industry is becoming nonexistent. With decreasing fusion program support, it is difficult to justify private investment as well. Continuing concern over future year budgets and the apparent low priority of fusion in the national picture further underscore the seriousness of the current trends.

In spite of the negative trends, there have been instances where industry has recently made sizeable investments in fusion. One such example is TRW's investment in MFTF-B, where industrial participation is growing, in spite of the decreasing budgets.

Program elements which foster industrial participation are (1) a sharing of the project mission with industry, and (2) a plan for continued industrial involvement, beyond any immediate hardware goals. Lawrence Livermore National Laboratory has subscribed to such a concept and has provided for an industrial role in the face of sharply decreased budgets. We urge that the entire national fusion program adopt a similar attitude wherein industrial participation is an equal goal to scientific and technological success.

Question 7

Given the large coil test program at the Oak Ridge National Laboratory, and various superconducting magnet development work for accelerators, what intermediate R&D, that is, R&D between now and the time that an ignition device using superconducting coils would be ordered, would be needed to keep U.S. industry competitive with other nations?

Answer

The development work for the LCT program is over and there are no apparent near-term programs for S/C fusion magnets. The accelerator magnets are more production than design and development oriented. The U.S. S/C magnet groups in industry are now facing layoffs even with some accelerator targets. There are significant technology differences between fusion and accelerator applications with the fusion application being much more challenging.

Areas where development programs would help U.S. industry to remain competitive are in higher current density magnets, quantifying the degree of stabilization required, bringing new conductors out of the laboratory and into the commercial arena, and in radiation resistant materials. An advanced magnet development program is warranted for the dual purpose of continuing progress in magnet technology and for maintaining the currently still excellent U.S. industrial capability.

Question 8

Assuming that a successful lower-cost ignition device is built and operated, then, what would be the next major step on the path towards a peaceful application of fusion energy? How might the next major step differ if a higher cost ($1.5 billion) ignition device were built five years later assuming there had been no prior experience with a lower-cost device?

Answer

From today's base of understanding, the next major step in the development of fusion is the demonstration and study of ignition. The process of energy transfer under ignition conditions is the last major science issue in fusion and must be addressed in an experimental device.

The question posed above deals with whether to build a special purpose ignition device or to build a device capable of studying both ignition and the engineering issues to follow. We strongly recommend that a separate ignition device be built for the following reasons:

(1) A special purpose ignition device would be small, inexpensive (less than $200 million), and could be built within the next 5 years.

(2) A single step Engineering Test Device would have to address physics and engineering issues simultaneously, making its design uncertain, with attendant high cost.

(3) The size, performance and hence the cost of the Engineering Test Device would be materially affected by the results of ignition testing.

(4) Within the current budgetary and political climate, it is unlikely that a full Engineering Device, with costs above $1.5 billion, could proceed, but a small, $200 million ignition device could, thus saving valuable schedule.

Question 9

Can anyone tell us how other on-going materials research that is being performed under other government programs is being incorporated in the fusion program?

Answer

Fusion relevant materials research is being performed primarily in the fission program. Communication is very good in most areas. Fission and fusion share a common materials technology base, we share people, and in many cases fusion projects are supported by fission people from governmental agencies and private industry. In particular, the work on structural alloys and insulators from the fission industry is well integrated, as are magnet materials, coolants, corrosion, radiation resistant materials and remote handling. Research in the aerospace industry on advanced materials is less well integrated, but a number of aerospace industry participants are bringing those material technologies into fusion as well.

Appendix A to Question 4.

I. Executive Summary of TMX-U Results, as of February, 1985.

PLASMA CONFINEMENT

Axial Confinement

- Under conditions of strong plugging, axial confinement increases to 100 ms.
- The 0.5-kV thermal-barrier potential profiles have been measured with 1.5-kV central-cell ion-confining potential.
- In accordance with theory, strong plugging requires neutral beams and two ECRH power sources.
- Strong plugging is lost at central-cell densities above 1.2 x x 10^{12} cm^{-3}. Modeling indicates that higher density plugging will require a reduction of radial transport, a slower density buildup, and additional barrier pumping.
- Weaker plugging with $\tau_{||} \sim 5$ to 10 ms occurs for densities up to mid-10^{12} cm^{-3}. A thermal barrier may not be playing a role.

Radial Confinement

- Ion radial transport produces axial electron loss current, which is used to measure the nonambipolar radial ion transport time $\tau_\perp$. Nonambipolar loss dominates radial transport. Ambipolar transport in the core is, at most, comparable to nonambipolar; it can be as small as zero to within experimental error.
- An empirical scaling $\tau_\perp$ (ms) = $3\,\phi_e^{-2}$ (kV) has been observed over a wide variety of conditions. Here ϕ_e is the central-cell plasma potential. This $\tau_\perp$ value is a factor of 1.5 to 5 below calculated resonance transport rates and has only roughly the same scaling with ϕ_e.
- At low potential, with grounded end walls, $\tau_\perp$ rises to 100 ms.
- Transport is markedly reduced by floating ring-shaped end-wall plates that are thought to reduce the radial electric field.

- With both end plugging and floating plates, we have simultaneously reduced axial and radial transport so the central-cell density rises in the scale of a few ms until plugging is finally lost.

Energy Confinement

- TMX-U has obtained global energy confinement times of up to 2.9 ms at a central-cell density of 6×10^{12} cm^{-3} ($n\tau = 1.7 \times 10^{10}$ cm^{-3} s.) This high-density operation was probably not with thermal barriers. It compares with the design goal of 4 ms at 2.3×10^{13} cm^{-3}.
- In the central cell, ion energy confinement is limited by charge exchange and electron drag. When combined they give a τ_{Ei} of $\cong$ 2 to 5 ms that is fairly insensitive to density.
- The plug sloshing-ion lifetime (1 to 8 ms) is mainly charge-exchange-limited.
- With low density operation, a plug-to-central-cell electron temperature ratio of 2 to 5 supports the existence of a thermal barrier.

END-PLUG PHYSICS

Hot Electrons

- Hot electrons have average β values ot 15% with no evidence of a beta limit.
- Mirror-confined hot-electron densities of up to 10^{12} cm^{-3} have been produced with hot tail-electron densities up to 5×10^{11} cm^{-3} and $E > 100$ keV.
- The plug hot-electron energy lifetime is 10 to 100 ms. The 10-ms time is a lower limit determined during heating, assuming total ECRH absorption, and the 100-ms time is from diamagnetic loop decay after the ECRH is turned off. Microwave and x-ray emissions indicate that energetic electrons persist for 0.9 s, until the magnetic field is turned off.

- A hot-electron density fraction exceeding 40%, as determined by radiometry, diamagnetic loops, and interferometry, is required for strong plugging. Theory predicts a fraction of 80% is required. X-ray data above 100 keV fit to a Maxwellian temperature indicate that 35% of the electrons are hot.
- From the rate of rise of the diamagnetism signal, the ECRH conversion efficiency to hot electrons is up to 42% of total antenna power. Second harmonic heating efficiency is as high as 70%.
- Experiments in which antenna aiming was varied indicate that directed microwave heating produces hot electrons more efficiently than cavity-mode heating does. This conclusion is supported by observations of little diamagnetism, with only fundamental heating power and little correlation between x-ray temperature and cavity power.

Sloshing Ions

- The sloshing-ion midplane density is up to 80% of total plug density.
- Sloshing-ion angular distribution remains peaked close to the injection profile, changing somewhat with the loss of plugging.
- Lifetime $\tau_s \sim 1.0$ to 1.5 ms with plugging and ~ 3.0 to 4.5 ms (up to 8 ms in some cases) without plugging, possibly because of a change in neutral gas shielding.

Barrier-Trapped Ions

- Barrier-trapped ions densities of 10 to 90% of the total plug density are present, depending on the experimental parameters. They are fueled by the collisional filling of passing central-cell ions, the ionization of gas, and charge exchange on sloshing ions.

CENTRAL-CELL PHYSICS

Central-Cell Fueling

- Central-cell particle fueling is with localized gas-puffing (gas box). Biased plates at the plasma edge are being developed to inject low-energy neutrals.

- The end fan for Faraday cups measured 20 to 35% of the input central-cell fueling gas. An additional 10 to 40% is calculated to be absorbed in the gas-box walls. The remainder is presumed to be pumped by the titanium-gettered walls.
- High central-cell density requires high gas feed.
- A low-energy neutral source for central-cell fueling has been demonstrated in initial experiments.

Neutral-Beam Heating

- The highest β-value is 5.2% averaged over the entire 25-cm radius, or 12.4% peak on axis with a 17.5-cm radial profile.
- Neutral-beam, central-cell-injected ions remain localized axially near the injection location. The high fueling gas density keeps them from filling the entire central-cell volume by collisional relaxation.
- Measured end-loss ion temperatures are higher than central-cell ion temperatures calculated by Coulomb collisions with beam-injected ions.

ICRF Heating: Double-Half-Turn Antenna

- Antenna radiation efficiency is as high as 90%. Diamagnetic loop measurements indicate that 20 to 40% of the radiated power is absorbed by ions.
- Average perpendicular ion energies of 1.5 keV have been created at a density of 2×10^{12} cm^{-3}.
- A 30- to 50-kW ICRF power threshold is observed for ion heating. The measured density and diamagnetic axial profile and end-loss analyzer indicate that the heating is anisotropic.

ICRF Heating: Slot Antenna

- Power-balance studies indicate that 40 to 100% of the slot antenna power is coupled to the halo plasma, raising the halo electron temperature to 80 eV.

mpurities

- The impurity radiation power is small. Central-cell impurity concentrations are low: carbon (0.9%), oxygen (0.9%), and nitrogen (0.5%).
- Carbon is produced from the walls, and oxygen and nitrogen are associated with neutral-beam injection.

MODELING

- The Zero-D multispecies and multiregion rate code TREQ incorporates classical processes and empirical radial nonambipolar transport. The strong heating model does not yet describe the loss of plugging. TREQ models steady state and transitions induced by the turn-off of transport (floating of potential control plates). TREQ is being employed to guide experiments.
- Radial Fokker-Planck code modeling of central-cell neutral-beam injection experiments agree with radial profile measurements of hot ions and total electron density. This work shows that the hot ion core is shielded by a cooler halo plasma.
- Bounce-averaged Fokker-Planck code calculations of central-cell ICRF heating agree with measurements of ion end-loss current, pressure anisotropy, and energy confinement time.
- Hot-electron Fokker-Planck calculations have guided TMX operation in locating high ECRH efficiency regimes at low density.
- Calculations of neutral gas flows have aided vacuum-system design modifications that allow TMX-U to operate with gas pressures that are sufficiently low for thermal barriers to be created.

STABILITY

- TMX-U operates MHD stable.
- Low-frequency fluctuations ($f < 1$ MHz) having features similar to the dissipative trapped-particle mode are sometimes observed.

- As predicted by theory the end-cell Alfven ion-cyclotron mode that dominated the TMX Experiment is not observed in TMX-U with the sloshing beams.
- Weak loss-cone modes are observed at sloshing-ion densities above 3×10^{12} cm^{-3}. These fluctuations do not affect central-cell confinement, and only rarely have they correlated with a reduction in sloshing-ion densities.
- Microwave emissions consistent with whistler and cyclotron maser instabilities are observed intermittently; however, ECRH efficiencies remain high and hot-electron confinement is affected only weakly, if at all.

TECHNICAL DEVELOPMENT

- Vacuum technology developments enable TMX-U to operate with neutral pressures below 1×10^{-6} Torr external to the plasma. Gas density is controlled by wall conditioning, titanium gettering, and the reduction of external gas inputs.
- TMX-U operates a 24-coil magnet system that is energized by 36 MW of primary power. Magnet misalignments are small, as indicated by rotating coil measurements, plasma-gun photographs, end-to-end electron-beam propagation measurements, and potential control-plate experiments.
- TMX-U operates with 24 neutral-beam injectors. The system generally operates with 100 atom amps (1 MW) injected into each end plug and 170 atom amps (1.7 MW) injected into the central cell.
- Four 28-GHz to 50-ms gyrotrons deliver 200 kW of power to oversized quasi-optical waveguide systems. To produce energetic mirror-confined electrons, a focusing twist reflector is used in the second harmonic resonance. A Vlasov-type polarizing reflector is used for fundamental heating to generate the confining potentials.

- Two central-cell ICRF systems that deliver 200 kW of power are operational. One employs a double half-turn antenna, and the other employs a slot antenna.
- A five-plate potential control system on each end of TMX-U is used to reduce nonambipolar radial transport and for diagnostic measurements. Each plate is segmented into quadrants and extends to a central-cell radius of 20 cm.
- Measurements of plasma parameters are carried out with 27 instruments that acquire 329 channels of data. In addition, 246 machine performance parameters are recorded.

II. Summary of Progress Since February 1985

- Barrier pump beams have been installed and observed to pump sloshing ions, as expected, in unplugged operation. In plugged operation, the sloshing ion lifetime is shorter, so the pump beams have not been observed to pump.
- The cause of the reduction in sloshing ion lifetime during plugging has tentatively been identified and corrective measures are currently being installed in the machine. These deal with the direction and location of injection of the neutral beam and ECRH power.
- Two new important potential diagnostics are now operational and routinely being used to determine extrema of the axial potential profile.
  - A time resolved end loss energy analyzer gives the history of the plugging potential. This has confirmed the existence of thermal barrier potential dips at both ends at the instant of loss of plugging. It also has revealed plugging outside the conditions anticipated in the thermal barrier model. These will test the theoretical understanding and may point to new plugging techniques.

  - A heavy ion beam probe now measures time history of the central cell potential. This is used in conjunction with the end loss analyzer to fix potential profiles.

- Floating of end wall potential control plates, installed as a technique for reduction of ion-selective radial transport, have been shown to reduce the central cell potential, as expected. With plate floating, the central cell density rises rapidly to the point of loss of plugging, indicating a sharp reduction in ion radial loss. Recent ion source measusrements have determined that the transport of ions and electrons is smaller than that of ions alone.

- The hot electron fractions in the plugs have recently been determined to lie in the range of 60-80%, as had been expected theoretically for thermal barrier formation.

- The central cell is being fitted with a double ICRH antenna array that, in conjunction with a new midplane gas box, will permit fueling the central cell in a way that provides more efficient heating of ions.